



Three Essays on Race and Politics

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Three Essays on Race and Politics

ABSTRACT

Understanding how race shapes the lives of individuals and transforms institutions is central to social science. Yet, for many scholars, race is widely understood as a fixed and monolithic category that is resistant to manipulation. As a result, making causal claims about “immutable characteristics” such as race or ethnicity has been strongly discouraged by statisticians and experts of causal inference. In contrast to previous literature, I propose a different framework that, in some cases, reconciles race and causation. Using a lab experiment and observational data about the urban uprisings of the 1960s, I test whether racialized and politicized cues from a subordinate group (in this case, blacks) can change psychological, behavioral and attitudinal measures among a dominant group (in this case, whites).

Looking at more than 750 violent protests that flared up in black neighborhoods across the United States, I examine whether increased exposure to signals of black unrest is associated with decreased support for the Democratic party. In the 1964, 1968 and 1972 presidential elections, I find a strong negative relationship between exposure to civil unrest and the county-level Democratic vote share. I find a similar negative relationship between exposure to violent protests and Democratic vote share in congressional elections between 1968 and 1972. Finally, I find that in counterfactual scenarios of fewer violent protests the Democratic presidential nominee, Hubert Humphrey, would have beaten the Republican nominee, Richard Nixon, in the 1968 election.

In the lab experiment, I test how exposure to images of politicized and armed white

and black men changes psychological, behavioral and attitudinal measures among subjects in the dominant (white) group. Methodologically, this study investigates the degree to which at least some aspects of race are better operationalized as variable, divisible, continuous and responsive to manipulation. Substantively, this experiment also attempts to assess the degree to which media representations of violence and politics might increase the salience of ethnic/racial identities, particularly in a dominant group. In the context of the 1960s urban uprisings, such a result might help explain why a significant subset of white voters switched away from the Democratic party, that had become identified with black interests, and towards candidates promising “law and order.”

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TO MY PARENTS, EILEEN AND BERNARD. IN A CHILD, THEY NURTURED A BOUNDLESS
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No causation without manipulation.

Holland (1986)

1

Reconciling Race and Causation

1.1 INTRODUCTION

For many social scientists, making causal inquiries about the role of race and ethnicity lies at the core of key research questions. Do majority-minority districts maximize substantive representation? How does racial priming in political advertising change voting behavior? Did Barack Obama's race cause him to lose votes in the 2008 presidential

election? Researchers from fields as disparate as labor economics, race and ethnic politics, and public policy have for years focused their exclusive attention on the causal role of race or ethnicity. This has been the case even if the “causal” claim (or language) is not explicit.

On the other hand, scholars of causality have long warned against making any kind of causal inference on the basis of race or ethnicity (Holland, 1986, 2003; Winship and Morgan, 1999). Why? Race is commonly understood as an immutable or unchanging characteristic. For centuries, societies have categorized people into different racial and ethnic groups, and a person’s race is generally thought to be resistant to manipulation. Within this fixed conception, race is defined at birth (if not earlier), largely determined by biology and impossible to assign randomly – the key tool for experimental causal inference. Furthermore, because factors like education and class are intimately linked to the distinct historical experiences of each racial and ethnic group, any attempt to isolate a single causal effect associated with race is likely to be deeply confounded. Experts on causality have thus warned that making causal inferences about race or race-based variables is usually a misguided enterprise.

Despite the proscription against causal inference with immutable characteristics, over the last two decades a growing body of scholarship, across a wide range of disciplines, has successfully made causal claims about effects of race. How is this possible? We show that, under specific conditions, it is possible to make causal claims about effects of race, ethnicity, or other seemingly fixed attributes. In particular, we argue that the “immutable characteristics” framework is too narrow in how it defines race and in how it defines the relevant units of analysis. First, we argue that race should be defined not as an “immutable characteristic” but rather as a composite variable that is both divisible and has mutable

elements.¹ This conceptual shift opens up important methodological possibilities. When understood as a composite variable, like socio-economic status, mutable elements of race can be disaggregated, manipulated and studied for causal effects.

Second, the prohibition on causal inference with regard to “immutable characteristics” presupposes that the effects of race or ethnicity only happen to the relevant racial or ethnic subgroup. By contrast, in many scholarly studies of race, particularly racial discrimination, the relevant unit of interest is often a person or institution, such as an employer, that need not be part of a specific racial subgroup. In short, the “immutable characteristics” framework ignores a critical class of subjects or units that are essential for estimating effects of race or ethnicity. Incorporating this broader range of effects and units further expands the allowable scope of causal inference when measuring effects of race and ethnicity.

Thinking more flexibly about these considerations opens up the possibility of making causal claims about seemingly immutable characteristics like race or gender. To be clear, this is not to say that making these kinds of causal claims is possible in all instances; what we demonstrate in this paper is that there are some limited areas in which making causal claims about race or ethnicity is possible (and has been successful in the past). These include (1) studies that measure the effect of exposing an individual or institution to some racial or ethnic cue and (2) studies that disaggregate race into constituent pieces and attempt to measure the effect of some mutable element of race within a single racial group. The later sections of the paper develop these ideas through two empirical examples. Throughout, we draw attention to those social science studies that have been

¹Although our focus is on race and ethnicity, much of the framework we present also applies to gender, sex, sexual orientation and other seemingly immutable characteristics.

successful in meeting these requirements. In sum, we provide a framework for making causal claims about race and ethnicity that unifies a diverse body of past research into two coherent approaches.

This article proceeds as follows. In Section 1.2 we briefly review how race has been operationalized in prior scholarship. In Sections 1.3 and 1.4, we explain briefly the potential outcomes framework and lay out the key problems involved with making causal inferences about race or gender. Second, we develop the fact that race brings with it different ways of thinking about experimental units, treatment regimes, and the composition of appropriate treated and control populations. Tying these threads together, we then develop a framework that unifies work from a wide range of disciplines into two types of studies: (1) exposure studies and (2) within-group comparison studies. Throughout, we analogize to effective experimental designs and point to successful social science research, in the process clarifying how race-based variables can — and cannot — be used by applied researchers in extracting causal inferences from observational studies.

1.2 DEFINING RACE

Broadly, two theories of race have dominated prior scholarship.² The first tends to view race in largely biological terms and to categorize populations by regions of ancestry and

²Though race is often defined as a biological inheritance and ethnicity as a cultural inheritance, in this article we use the terms “race” and “race and ethnicity” interchangeably. We do this for several reasons. First, many groups, such as U.S. Hispanics, in some contexts are categorized as a racial group and in others as an ethnic group. Second, within political science, the term of choice often varies by region and sub-discipline. For example, the term “ethnic minorities” is used by many European social scientists to refer to groups that would be considered racial minorities within the United States. Third, as the field of epigenetics demonstrates, biological, environmental and cultural influences interact in ways that can make drawing clean lines between race and ethnicity challenging.

phenotype. Though the biological concept of race has its roots in the work of eighteenth century naturalists classifying isolated populations, some contemporary genetic research supports the idea that people with similar geographic backgrounds also share clusters of common genes that correspond roughly to modern racial categories (Blank, Dabady and Citro, 2004; James, 2008). The second approach emphasizes the weak scientific basis for racial categories and argues that race is best understood as a social construction (Appiah, 1986; Omi and Winant, 1994; Zuckerman, 1990). In contrast to the biological approach, the social construction approach emphasizes that distinctions between so called races and the importance ascribed to various genetic or phenotypic traits are the products of social forces. These social forces include a complicated amalgam of cultural, historical, geographical and legal influences (Appiah, 1986; López, 1994; Loury, 2002; Holland, 2003).

How race is defined determines, in large part, how it can be operationalized in scholarly research. The “immutable characteristics” framework of race and causal inference builds on the biological conception of race and logically argues that genetic and phenotypic traits are generally not amenable to manipulation. Though leading scholars of causal inference like Holland (2003) recognize race as a “socially determined construction with complex biological associations,” the methodological implications of this conception are not developed. We build our approach on the social construction framework and show that this approach can be exploited to allow meaningful causal inference about race. Unlike genetic and phenotypic traits, the social amalgam of race can be disaggregated into constituent elements, some of which can be reasonably manipulated to estimate causal effects.

1.3 CAUSAL INFERENCE AND POTENTIAL OUTCOMES

A brief overview of the Rubin potential outcomes framework helps contextualize the following discussion. (The literature on this topic is voluminous — e.g., Rubin (1974), Holland (1986), Angrist, Imbens and Rubin (1996), Rubin (2005) — and we attempt in this discussion only a bare-bones introduction.) At its core, a causal inquiry involves unpacking the effect of some treatment on some outcome.³ Does a vaccine cause people to live longer? Is a worker training program effective in helping people go back to work? In all of these cases we see (1) a unit of analysis, (2) a manipulable treatment, and (3) a specific outcome.

The “fundamental” problem of causal inference is, however, that we can never observe the difference between these two potential outcomes for any individual unit (Holland, 1986; Rubin, 1978). That is, once we assume no convoluted theories involving time travel, a single unit simply cannot receive both the treatment and the control at the same time. This problem extends to all kinds of inquiries – for example, when testing different medicines, or seeing the effects of a work training program – but it becomes particularly vexing when it comes to immutable characteristics. After all, a person cannot experience the world as being only black and also as being only white, or as being only Native American and as being only Maori, and to think otherwise raises strange hypotheticals. This is an important point to which we return throughout.

In lieu of trying to estimate an unobservable true treatment effect, those interested

³We note that many interesting questions can be asked and answered with non-causal questions, and much of what we say here (particularly with regard to disaggregating components of race) applies equally to non-causal research designs.

making causal inferences usually estimate some version of the *average* treatment effect, that is, the difference between the outcome means in treated and control populations. An obvious problem is, however, that differences in the outcome variable could be due to inherent differences between the treated and control populations, a problem that some refer to as selection bias (Angrist and Pischke, 2009). For example, we should not be surprised to see that workers who have signed up for a worker training program are more successful in getting jobs – but we also should not be surprised that they are also more ambitious and better educated than non-trained workers, two attributes that would also result in more favorable employment decisions.

The problem is solved in some circumstances by comparing only similarly situated treated and control units. To get at a satisfactory estimate of the average treatment effect, we would like our treatment and control groups to be similar across all background variables that could affect both the probability of receiving treatment or the eventual outcome. such that that the only difference between the two groups is that one received the treatment and the other did not. This is known as the ignorability assumption. Put another way, the treatment assignment must be independent of the potential outcomes in order for us to assume that the two groups are similar enough to extract causal inferences.

Many empirical efforts are geared toward satisfying the ignorability requirement – that is, to make the treated and control populations as similar as possible so that the treatment regime could be assumed to be random. By far the easiest way to satisfy the ignorability requirement is simply to assign the treatment randomly – for example, by conducting a randomized experiment. (We discuss some successful experimental designs below; more general discussions are found in Holland (1986) and Imai, King and Stuart (2008).)

However, because randomization is rarely an option for political scientists (especially, as we note, for those studying race or ethnicity), researchers have turned to matching or controlling for observed variables to satisfy the ignorability assumption that the treated and control groups are identical on background covariates (Dehejia and Wahba, 2002; Sekhon, 2009). We discuss the utility of these techniques for race and politics scholars, below.

1.4 HOW POTENTIAL OUTCOMES BREAK DOWN WITH RACE

Given these key basics of the potential outcomes framework, we now turn to exploring why race and ethnicity present such causal bugaboos. The existing literature has identified two key problems (Greiner and Rubin, 2010): (1) biological elements of race are resistant to manipulation (yielding experimental analogies unidentified), and (2) because race is generally understood to be “assigned” at birth (or conception), the host of characteristics for which most social scientists control (e.g., education, income, etc.) occur after the treatment is assigned and therefore have the potential to introduce post-treatment bias. In addition, we introduce a third problem: (3) building on the idea that race should be viewed as an amalgam of characteristics or a “bundle of sticks,” researchers often misclassify what the race variable actually represents, thus confusing the nature of the “treatment.” We address these three problems and their implications for potential outcomes in turn.

PROBLEM 1: RACE CANNOT BE MANIPULATED. Making causal inferences usually demands a neatly defined, manipulable treatment variable, one that can be easily

documented and manipulated by researchers.⁴ Holland (1986), for example, famously admonishes “No causation without manipulation” to bring attention to the idea that all pertinent potential outcomes must be defined in principle in order to make causal estimates possible in practice. Further, to define all potential outcomes, one must be able to conceptualize an experimental analogy that would lead to the possible outcomes. In other words, as Holland puts it, “causes are only those things that could, in principle, be treatments in experiments.”

This idea of a manipulative treatment is echoed by others like Cook, Campbell and Day (1979), who argue that “[c]ausation implies that by varying one factor I can make another vary”; Pearl (2000), who discusses at length the importance of an intervention in estimating causal treatments; and Gelman and Hill (2007), who warn that “a causal effect needs to be defined with respect to a cause, or an intervention, on a particular set of experimental units.”⁵

The biological dimensions of race and gender are, however, resistant to manipulation. (Imbens and Rubin (2010) refer to them as “currently immutable characteristics,” as

⁴The literature on this point is rooted as much in statistics as it is in philosophy and political thought – e.g., Locke (1847), Hume (2003), and Mill (1884). More recently, philosophers looking at the topic have advanced the idea that manipulation is at the core of a causal inquiry, and treatments such as Menzies and Price (1993) and Von Wright (1971) focus specifically on human intervention or action. Others, such as Hausman (1998), have critiqued this literature as putting too much emphasis on human agency, which has the effect of overlooking the natural or non-human manipulations and interventions that can occur. Holland (1986) provides an informative overview of this literature.

⁵We may think that the experiences of mixed-race people may solve this problem. After all, people who are mixed race routinely “pass” as members of one group and then also as members of another group, and a rich and varied literature (scholarly as well as popular) has developed around how multiracial people self-identify (Faulkner, 1990; Schuyler, 1971; Halsell, 1969; Griffin, 1996; Gates, 1997; Kim and Lee, 2001; Hochschild and Weaver, 2010). From a causal perspective, however, mixed race people represent those for whom a third kind of “treatment” has been administered – a mixed race treatment. Thus, although the experiences of these sorts of individuals may be informative – and illustrates that the neat disaggregation of races into categories is never easy, a point we develop below – much of this discussion applies to multiracial individuals just as it does people who self-identify exclusively as black, white, Asian American, etc.

future scientific innovations may dramatically ease the effort required to change to certain biological aspects of race or gender.) Treatment by race and gender also suffer from the problem that it is difficult to think about appropriate counterfactuals. We can imagine how someone lives their life as an African American; much more difficult is imagining what experiment one would design to manipulate the person's race (and *only the person's race*) to check its effect on some outcome. Thus, not only is randomization, the most elegant solution to the fundamental problem of causal inference, beyond the reach of many scholars focusing on the effects of race or ethnicity, but it is difficult in many instances to even conceptualize what an ideal experiment would look like. (We discuss exceptions below.) Ultimately, as Angrist and Pischke (2009) point out, research questions for which there are no experimental analogies (even hypothetical ones, in a world with unlimited time, research budgets, and mildly omniscient powers) are fundamentally unidentified questions.

Within the causal literature, the immutable (i.e., resistant to manipulation) nature of race and gender has led many to cite race and gender as attributes for which causal inferences are impermissible (e.g., Holland (1986); Rubin (1978); Gelman and Hill (2007)). As noted by Holland (1986): "For causal inference, it is critical that each unit be potentially exposable to any of the causes. As an example, the schooling a student receives can be a cause, in our sense, of the student's performance on a test, whereas the student's race or gender cannot." A more specific admonishment on the topic of gender-based causality is given by Rubin (1978):

[C]onsider the causal effect of sex (male-female) on intelligence. What are the actions to be applied to each experimental unit that define the treatments? Are we to give hormone shots beginning at birth and surgically

perform a “sex-change” operation, or at conception “change” Y-chromosomes and X-chromosomes? Even if an “at-conception X- for-Y chromosome change” becomes possible, presumably there will be several techniques developed for effecting the change with potentially different causal effects. Without treatment definitions that specify actions to be performed on experimental units, we cannot unambiguously discuss causal effects of treatments.

PROBLEM 2: WITH RACE, EVERYTHING IS POST-TREATMENT. A second problem with conceptualizing well-defined potential outcomes is that a person’s race is “assigned” by a combination of social and biological processes at conception or birth. Thus, the host of background covariates that social scientists usually control for or match on (e.g., education, income, age) are determined *after* a person’s race is assigned.

Taking into account things that happen after the treatment happens or is administered has the potential of introducing post-treatment bias, a pervasive problem within observational social science research (King, Keohane and Verba, 1994; Rosenbaum, 2002). To use a common example, suppose that we are interested in the causal effect of smoking on death, and have a population of randomly assigned smokers and randomly assigned non-smokers. Should we control for lung cancer in the final analysis? Probably not: lung cancer is not only highly predictive of death, but it is also a direct consequence of smoking – probably *the* key consequence. If we controlled for lung cancer, the effect of smoking on death would essentially be null, biased downward by the fact that we have controlled for its primary consequence. Race is obviously different from smoking, but the post-treatment issue applies with equal or greater force: race affects deeply how a person is raised and educated, what kinds of employment opportunities (and hence employment experiences) he or she will have, and what kind of cultural and social attitudes he or she

will bring to the table. Including any of these attributes would therefore affect our estimates of the causal effect of “race.”

Although perhaps unsatisfactory to many applied researchers, the most appropriate initial approach is to drop any post-treatment variables from an analysis (King, 1991; King, Keohane and Verba, 1994; King and Zeng, 2006; Gelman and Hill, 2007). In this context, any factor, attribute, personality trait, or personal or professional experience that could potentially be a consequence of race should be dropped – a practice that would eliminate most of the variables included as standard controls by social scientists. For example, if we were studying the effect of race on employment, we would not control for anything directly impacted by the subject’s race, e.g., age, education level, income, felon status, zip code, health status, etc. The right-hand side of a regression would simply include race and, possibly, sex.⁶ We note that this strategy implies that the researcher is interested in the *total* effect of race (VanderWeele and Hernán, 2012) – which might not be satisfying to both researchers (and reviewers unfamiliar with the causal literature). However, there may be instances where this is not the case, and the researcher is interested in the effects of constituent components of race; we discuss this below. This kind of research design still also fails to address the critique above that experimental analogies are undefined.

Even aside from the post-treatment issue, we note two further problems with controlling for race-related covariates: (1) common support problems and (2) problems

⁶Sex, which is also assigned at birth, is one of the few standard control variables that could be construed as being pre-treatment or, at the very least, assigned concurrently with the treatment. Evidence suggests, however, that sex ratios can vary by latitude, religion, ethnicity and other factors collinear with race (Guttentag and Secord, 1983; Navara, 2009). Other possibly pre-treatment factors (e.g., genotype) are discussed in VanderWeele and Hernán (2012).

with multicollinearity. The common support problem arises when researchers include attributes that vary according to race (e.g., welfare status, participation in programs like Head Start, diseases such as sickle cell anemia or Tay Sachs). Because these traits vary almost exactly according to race, it becomes difficult to find non-minority (or minority) counterparts with which to compare the population of interest. (For example, it would be hard to find a sizable group of whites who have sickle cell anemia (Thomas and Zarda, 2011).) Collinearity becomes a problem when variables or effects vary so closely with race so as to result in (the most extreme case) unconverged calculations of point estimates. The lack of variance in the background variables may also result in small changes having a large impact on the coefficient estimates – thus, standard errors may be large and lead researchers to assume no treatment effects when treatment effects do in fact exist.

PROBLEM 3: RACE IS ACTUALLY A MULTI-FACETED TREATMENT. The prior two methodological problems associated with race are well-known and well-cited in the causal inference literature. We now introduce a third problem, which is the fact that race is rarely a single, easily defined measure (or treatment). To the contrary, the work of race and ethnic politics, sociologists, anthropologists, and critical theorists (e.g., Appiah (1986); López (1994)) has emphasized repeatedly that race is a complicated amalgam of things like skin color, cultural traits, physical attributes, region of ancestry, and education – many of which vary across members of a single group. In other words, race is a composite of many component pieces; metaphorically, it is a “bundle of sticks” (Figure 1.4.1). Researchers could never assign the full bundle of factors that constitute a racial identity to some subjects while assigning others to a control; neither could they

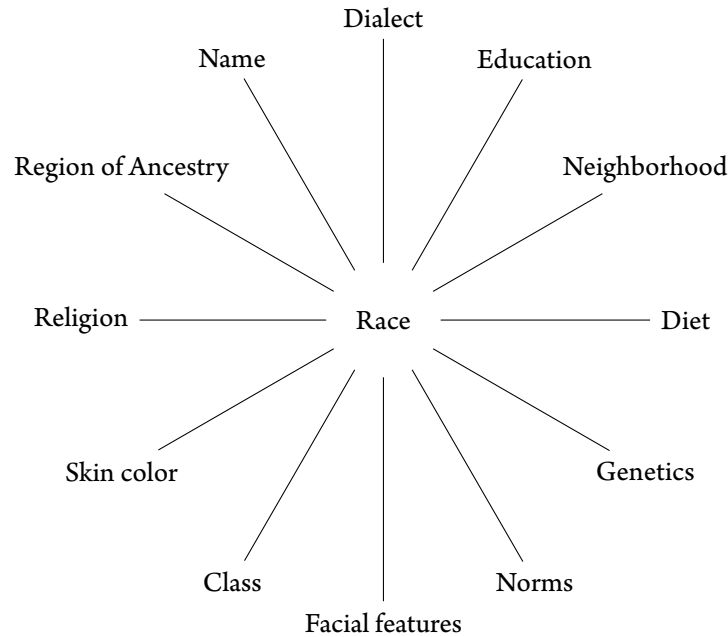


Figure 1.4.1: Some Characteristics Associated with Race or Ethnicity

safely assume that someone identifying with one racial group uniformly shares all components of that racial categorization. It would, for example, be strange to assume that a Mexican American from Los Angeles shares all component attributes of being “Latino,” or shares them to an equal extent, as a Puerto Rican American from the Bronx.

Despite the variable, multi-faceted aspect of race, most quantitative social science nonetheless represents race via a single indicator dummy variables (“0” if white, “1” if black), categorical variables (“1” if white, “2” if black, “3” if Latino, etc.) or percentages (e.g. percent of a majority-minority district that is black). The problem of using such monolithic measures of race is twofold. First, any statistical association will typically offer little or no insight as to which element of race is the key mechanism of action – be it skin color, education, ethnic slang, discrepancies in health measures, class, etc. Second, a

simple dummy in no way captures the fact that the treatment (“race”) can vary in its components quite radically from observation to observation – for example, our comparison between Mexican Americans versus Puerto Rican Americans. This represents possible ongoing violations of the stable unit treatment assumption (SUTVA), and calls into question whether an accurate estimate of a race-based treatment effect is actually possible. Quite simply, forcing something as complicated as race into simplified categorical variables potentially introduces serious measurement error.

1.5 RESEARCH DESIGN 1: TREATMENT AS EXPOSURE TO RACE

Although the problems cited by the causal inference literature can never be fully solved, they might in some instances be circumvented with the correct research design. We first consider a type of research design that we call “exposure to race” or exposure studies. These studies examine how subjects respond when exposed to some sort of racial signal or cue and, as such, might be more precisely called “exposure to a racial signal” or “exposure to a racial cue” studies. (These sorts of research designs have been described by Greiner and Rubin (2010) as those that look at the effects of “perceived race” and by VanderWeele and Hernán (2012) as those that look at discrimination.) Among the studies included in this group would be those that look at how voters respond when presented with advertisements showing black versus white candidates, or those that examine whether employers are more or less likely to interview job applicants with traditionally African American names.

In these sorts of research designs, (1) the treatment of interest is the exposure to a racial cue and (2) the unit of analysis is the individual or institution being exposed; both

alleviate the problems of race and causality. Using a simple employment example, suppose we are interested in racial discrimination against black job candidates in today's job market. We can easily think of an ideal experiment, which would take two applicants, one white and one black, and construct a job profile that is exactly the same for each applicant – except for some signal or cue (such as a name or picture) that one applicant is black and one is white. (Bertrand and Mullainathan (2004), for example, relied on distinctively African-American names to send a signal about the applicant's race.) The researcher would then send these job applications to employers and check the eventual hiring decisions. A difference would suggest that similarly situated blacks and whites are being treated differently, while no difference would suggest no discrimination. Again, key to this kind of study is that the unit of analysis is actually the prospective employer (*not* the prospective employee), and the treatment is the kind of name attached to the job application. Thus, the research design begins with well-defined potential outcomes, is operationalized via a clean experiment (or a clean experimental analogy), and has a precise moment of treatment. The causal impact of race and ethnicity is identified, alleviating the problems cited above.

EXPERIMENTAL EXPOSURE STUDIES. These kinds of audit or correspondence studies have been used experimentally to measure race and gender discrimination in a wide variety of fields,⁷ including labor economics, (Bertrand and Mullainathan, 2004),

⁷Pager (2007) provides a good overview of the literature, critiques, and methods. Although the exact methodology may vary, audit studies usually involve confederates or actors hired by researchers who are then randomly sent out to the field – for example, to different employers or to different lending agents. Partly in response to critiques about potential bias introduced by the confederates (Heckman and Siegelman, 1993; Heckman, 1998), correspondence studies were developed in which matched human applicants were replaced with matched pairs of “paper” applicants. Bertrand and Mullainathan (2004), as noted above, randomly assigned traditionally white and black names to otherwise similar resumes to assess how

sociology (Bobo and Johnson, 2004), psychology (Cosmides, Tooby and Kurzban, 2003; Boker et al., N.d.; Steele, 1997; Greenwald, McGhee and Schwartz, 1998). Within political science, a robust public opinion literature (Miller and Krosnick, 2000; Huber and Lapinski, 2006; Valentino, Hutchings and White, 2002; Mendelberg, 2001; Sniderman and Piazza, 1993; White, 2007) has exploited some variant of the exposure research design to estimate race-based causal effects. Sniderman and Piazza (1993), for example, leverage question order to find that the “mere mention” of affirmative action to white survey respondents provokes more negative feelings towards blacks. Mendelberg (2001) creates simulated television news experiments to assess how racial cues might prime racial attitudes among white voters. Within the participation literature, Butler and Broockman (2011) use distinctively black and white names to craft putative “constituent” emails to state legislators; emails from white “constituents” were more likely to be answered by white representatives. Within psychology, Kurzban, Tooby and Cosmides (2001) expose subjects to photos and text to simulate a cross-race conversation and Steele (1997) identifies how internalized racial stereotypes affect women and racial minorities by exposing them to racial and gender cues immediately prior to a mathematics exam. Kurzban, Tooby and Cosmides and Steele also demonstrate how the exposure model can address questions other than concerns about a discriminatory “decisionmaker” (to use the terminology of Greiner and Rubin (2010)).

To step back a moment, although all of these studies exploit different techniques –

such signals about the race of the applicant affected hiring decisions. More recently, Adida, Laitin and Valfort (2010) used a similar technique to measure employment discrimination against Muslims in France. Though scholars have viewed audit and correspondence studies as related, we note that *all* studies employing exposure to a racial cue should be viewed as related and part of a common literature on race and causation.

from simulated avatars to scenarios in surveys – the general approach is the same: randomly present a subject with information that differs only with respect to signals or cues about race. Though exposure to race is a useful shorthand, it is important to note that the treatment is never race in full (i.e., the whole “bundle of sticks”) but rather only an element of race such as name or physical appearance.⁸ Research designs of the exposure type thus have (1) a randomly assigned treatment, which is the racial signal or cue, and (2) a unit of analysis, which is the subject being exposed to the racial cue. And, as a result, we have (3) well defined potential outcomes and (4) a precise, well defined moment of the treatment is assigned. Accordingly, the causal role of race is appropriately identified, provided thought has been given to the specific experimental design.

OBSERVATIONAL EXPOSURE STUDIES. It is possible to import this research design to a wide variety of *observational* contexts involving how third parties react to once they are exposed to racial signals and cues. In this sense, we could use observational data to understand how mortgage lenders react to Asian American versus white borrowers (Sen, 2012), how juries react to Hispanic versus non-Hispanic death penalty defendants (Greiner and Rubin, 2010), how voters respond to political ads featuring black versus white actors, how universities respond to minority versus non-minority applicants, and how the U.S. government reacts to proposals submitted by minority-owned business in deciding to award contracts. In all of these instances, the interest lies in understanding how exposure to race changes or informs others’ opinions, behaviors, or attitudes – a fact that makes this kind of research design ideal for testing implicit bias or racial

⁸The exposure study approach could also be used to manipulate cues about characteristics beyond race and ethnicity.

discrimination (Greiner and Rubin, 2010; VanderWeele and Hernán, 2012).⁹

With observational data, researchers must be aware of two attendant issues. First, using observational data means that researchers lack the ability to manipulate the racial cues and signals received by the subject. It is therefore necessary to include those background variables in the analysis such that the only functional difference between the treated and control groups is that one group is exposed to minority, or other racial cues and that the other is exposed to non-minority racial cues. Second, and perhaps more helpfully, the exposure research design greatly lessens problems of post-treatment bias (Greiner and Rubin, 2010). To illustrate, suppose we are interested in whether a university accepts minority versus non-minority applicants at different rates – perhaps due to affirmative action but, perhaps also, due to invidious discrimination. The ideal experiment here would be to mimic an audit study and create identical applicants whose profiles differ only with regard to their race. The “treatment” would be administered to the admissions officer at the time he or she reviews the application packet. Anything that happens before is solidly pre-treatment and must be conditioned on; anything that happens after (e.g., decisions about financial aid, work study opportunities) would be post-treatment and should be dropped from the statistical model (Greiner and Rubin, 2010).

This discussion can be boiled down to one key idea: when possible, conceptualizing an

⁹Greiner and Rubin (2010) refer to this type of observational research as teasing apart the effects of “perceived” race (as opposed to actual race). We use different terminology and draw different analogies, but the research design we suggest here is comparable. Nonetheless, we move away from the “perceived race” language for two reasons. First, we think the best way to think about the “treatment” in these kinds of studies is not as perception but, instead as a signal about race. After all, in an experimental context, the researcher can manipulate the signal to which the subject is exposed but not what the subject actually perceives. Second, perceived race is rarely observed: what a subject perceives occurs within the confines of a mind and is opaque to researchers. As such, focusing on exposure to race rather than perception of race is preferable.

experiment or observational study as an exposure study greatly reduces both the theoretical and practical problems associated with making race-based causal inferences. Thus, applied researchers should think carefully about whether an exposure study could provide a well-suited analogy for their research questions and hypotheses. We also note that this is a research design that is particularly apropos to questions involving racial discrimination, disparate treatment, and priming.

1.6 RESEARCH DESIGN 2: ELEMENT OF RACE DESIGNS

Exposure studies offer a useful framework when individuals or institutions have been presented with some signal about race – i.e., we are interested in discrimination, bias, or disparate treatment. However, many research questions do not involve an external actor exposed to a racial cue: Why do African Americans suffer from increased rates of heart disease? Why are certain ethnic groups overrepresented in rebel militias? What explains the educational “achievement gap” between blacks and whites? In these studies, there is generally no treatment by exposure and no “decisionmaker.” (VanderWeele and Hernán (2012) refer to these studies as those focusing on “discrepancies.”) For scholars working on these sorts of topics, the primary research interest – and the appropriate units of analysis – lies in a particular racial or ethnic population itself. And these studies are particularly problematic in terms of having ill-defined potential outcomes and also having post-treatment bias problems.

For these sorts of questions, we suggest a different research design, one that exploits variation *within* a racial group to extract causal inferences. We call these “element of race” or “within-group” designs. This kind of research design disaggregates the “bundle of

sticks” discussed above and singles out a specific element of race that can be manipulated in an experiment (or observed to vary) within a racial group. By identifying a mutable element of race, it is possible to identify well-defined potential outcomes and to assuage potential post-treatment bias problems. The bundle of sticks thus becomes a blessing instead of a curse.

For example, suppose we are interested in understanding disparate educational outcomes for black versus white inner-city youngsters. A naive analysis would be to regress educational outcomes on race (possibly other control variables), taking the group of African Americans as the “treated” group and whites as the “control.” For all the reasons cited above, however, a causal estimate based on this research design would be (1) fundamentally unidentified and (2) biased by any inclusion of post-treatment variables. A better research design would be one that takes as its starting point the fact that the race variable captures a variety of factors, and, rather than conceive of black youngsters as a treated group and white youngsters as the control, identifies a trait highly collinear with race that is in theory manipulable. One example of such a trait might be neighborhood. With this in mind, we can re-cast the study as a *within-group analysis* where we compare black youngsters with mothers in one neighborhood to those black youngsters in a different neighborhood (for an example of this very approach, see the Moving to Opportunity experiments by Katz, Kling and Liebman (2001)). We could include white children, but the cross-race comparisons would only be useful for descriptive purposes, not for meaningful causal inference. The end result would be an identified causal effect that not only gives us valuable insight into the other descriptive findings but also narrows down the causal mechanism that explains disparate, race-based educational outcomes.

We note several distinct advantages to this research design over more naive cross-race regression approaches. First, limiting the unit of analysis to a single racial group and reconceptualizing the treatment as being something that varies closely, but perhaps not exclusively, with race allows for experimental manipulation (in theory or practice), thus avoiding the critique that no well-defined potential outcomes exist. Second, because the alternate treatment may be “assigned” post-birth, it also allows for the inclusion of all pre-treatment variables (confounders), including traits like mother’s education, health, nutrition, and early educational opportunities. To some extent, we are advocating treating simple biological race (as this is what the race variable now becomes) as a confounding variable that must be controlled for or conditioned on.¹⁰ Third, with enough data, conditioning on race before moving to a causal analysis resolves the common support problem; it might be difficult to find white matches living in similar areas as black children, but focusing on within-race variation will often resolve this problem.

Lastly, while it would be impossible to meaningfully assign all of the components of race as a treatment, disaggregating race allows for the investigation of an effect of a single “stick” or element of race – a much more tractable enterprise. For example, a randomized medical trial that incorporated more than one treatment, such as multiple changes in a diet, would be unable to distinguish which elements of the dietary intervention were therapeutic. Only by isolating a single change, for example swapping regular soda for diet soda, can a meaningful effect be isolated. Similarly, approaching race and ethnicity

¹⁰Here, the approach we are suggesting may in some instances be similar to an effects modification approach. Effects modification would be appropriate in instances where the treatment effect varies according to some different strata or subgroup (i.e., there are heterogeneous treatment effects that vary systematically by subgroup). Because the impact of the alternate, non-race treatment may vary according to subgroup, comparing the results between groups may also be useful.

through the lens of the “bundle of sticks” solves one of the most persistent problems associated with studying race or ethnicity: the difficulty of knowing what exactly is being estimated. Because causal (or even most descriptive) estimands aren’t actually race but various concepts linked to race, our approach allows researchers to clarify with more precision what is being estimated and how that quantity is linked to race.

LEVERAGING THE RIGHT KINDS OF ELEMENTS OF RACE. Which components of race would make for suitable treatments? This largely depends on both the research question and the researcher’s preliminary hypotheses; our advice is that more manipulable components of race are the most tractable, theoretically and practically. Consider Figure 1.6.1, which presents a hypothetical continuum of features that are strongly associated with race but that exhibit varying degrees of mutability. Facial features – such as the shape of one’s eyes or the contours of one’s nose – are fairly immutable, possibly changed through plastic surgery but certainly not something researchers could manipulate easily. (The boom in ethnic-oriented plastic surgery might present some interesting, if far-flung, experimental possibilities – Dolnick (2011); Survey (2004).) These sorts of traits are less useful for researchers, as they present the same conundrums as immutable characteristics do.

A better approach would be to pursue a treatment that is at once more mutable and more likely to be social constructs – e.g., name or neighborhood. A large literature in gender studies distinguishes between “sex” and “gender” where “sex” is defined as biological and anatomical while “gender” is defined as the product of psychological, social and cultural forces (see, for example, West and Zimmerman (1987); Deaux (1985)). Similarly, we suggest scholars attempt to distinguish between what might be

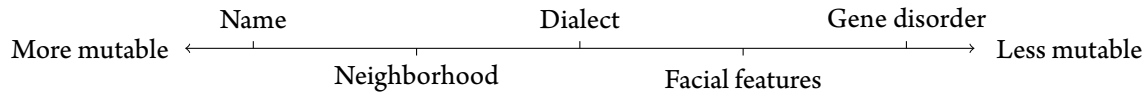


Figure 1.6.1: Mutability of Characteristics Associated with Race and Ethnicity

called “biological race” and “social race” (with the understanding that such categories can never be cleanly delineated) and focus on research designs that target social and environmental aspects of race. Names, for example, are not only quite mutable but provide a strong signal about racial or ethnic background (Chang et al., 2010; Washington, 2011). One could imagine an experiment in which new parents of the same race and socioeconomic background were randomly assigned to pick a baby name from one of two lists. One list would include names that are not strongly identified with the relevant racial or ethnic group and the other list would include names that do exhibit such an association. This kind of study could then assess the short- and long-run effects of a racially or ethnically specific name on outcomes like education or employment. The key point is that not all of the “sticks” are inherently immutable; neither is the whole “bundle” automatically assigned at birth. For applied researchers, perhaps the best strategy is to conduct a variety of within-group analyses manipulating various of these elements to more precisely isolate a causal mechanism.

STUDIES MANIPULATING AN ELEMENT OF RACE. A small number of experimental studies have begun using this kind of elements of race approach. For example, one element of race (i.e., one of the sticks in the bundle) is self-worth and self-assessment, which in turn are amenable to experimental manipulation. Walton and Cohen (2011), for example, randomly assigned college freshman a message that all college students struggle

to fit in initially but can ultimately succeed. Compared to the black control students, the black treated students exhibited sustained academic improvements over their college careers and later reported being happier and healthier than the black controls. Treated whites, on the other hand, exhibited no significant difference from control whites.

In the observational context, some studies have successfully leveraged additional components of race in order to extract surprising inferences. Cutler, Fryer and Glaeser (2005), for example, explore why African Americans suffer from higher rates of hypertension compared to whites. By more closely examining black subpopulations, they demonstrate that blacks whose enslaved ancestors survived the “Middle Passage” across the Atlantic exhibit higher rates of salt sensitivity compared with blacks whose ancestors were not enslaved (i.e., more recent African immigrants to the United States or the United Kingdom). A possible mechanism is that salt retention – a precursor to hypertension – enabled enslaved African to survive the deadly three-month sea voyage that constituted the Middle Passage. Thus, the appropriate treatment in this study was not race per se; it was treatment by the Middle Passage, a finding only made clear with within-group comparisons.

Another example is provided by Nisbett and Cohen (1996), which explores why white American men in the South exhibit higher rates of violence than white men in the North. Nisbett and Cohen identify and experimentally test cultural differences they hypothesize are borne of varying immigration patterns. Where a more conventional research design might have compared rates of violence between white and black men, Nisbett and Cohen attempt to disentangle the effects of race and norms by exploiting cultural variation between Northern and Southern white men. The standard cross-race approach takes the

appropriate units of analysis to be the person or person(s) of color and his or her white (or non-minority) counterpart.¹¹ Though cross-race comparisons are widely used in fields such as health and education, due to post-treatment bias and immutability, such comparisons are problematic when attempting to provide anything more than a descriptive analysis. In contrast to exposure studies that attempt to measure a contemporary effect of race as a signal, studies exploiting within-group variation are often attempting to identify some trait or quality assigned to members of a population at an earlier historical period.

1.7 EMPIRICAL EXAMPLE: EXPLAINING THE ACHIEVEMENT GAP

We illustrate some of these concepts via an empirical example taken from Fryer Jr and Levitt (2004), which explores the determinants of educational outcomes for white versus black children. Most literature in this field has found that black children consistently and strongly underperform on educational tests, despite researchers controlling for a variety of socioeconomic factors that could potentially affect educational outcomes. This has left policy makers and scholars puzzled as to how and why the gap between white and black children develops as well as how it could be lessened. In sharp contrast to the previous literature, however, Fryer Jr and Levitt found that the test score disparity between white and black kindergarteners almost entirely disappears after accounting for three factors: (1) participation by students' families in Women, Infants, and Children welfare programs

¹¹Of course, this is not the approach taken by all applied researchers. Some researchers, particularly in race and ethnic politics or in urban politics look at different measures – for example, the percent of a census tract that is minority. However, looking at minority versus non-minority populations does seem to be the general default approach.

(WIC), (2) whether the mother was a teenager at time of the child's birth (or more generally, mother's age), and (3) an amalgam measure of socioeconomic factors. Thus, Fryer Jr and Levitt conclude that socioeconomic forces are at play, and that they plausibly exacerbate over time, thereby continuing to disadvantage black students.

Table 1.7.1 demonstrates our replication of Fryer Jr and Levitt's core analysis, which used data from the Early Childhood Longitudinal Study (ECLS-K). The key outcome here, and in subsequent analyses, is the students' performance on kindergarden-level testing. (In the interest of full disclosure, we were able to replicate their results to approximately a tenth of a decimal for each coefficient, although we had different sample sizes due to different approaches to handling missing data; substantively, the results are all identical, and multiple imputation of missing variables does not change the results.) Like Fryer and Levitt, we break up the analysis into test scores of math and test scores on reading. In all instances, whites comprise the baseline group to which (1) blacks, (2) Hispanics (an exclusive category), (3) Asian Americans, and (4) "others" are compared.

Thus, when a simple analysis of race on educational outcomes shows that black and Hispanic students fare worse than whites, while Asians perform better (Model 1 and Model 4). Focusing on black-white differences specifically, the most interesting results come in the way of the scores on the reading tests (Models 4-8). Here, black students begin with the traditional disadvantage against whites – controlling for nothing except student race, black students on average achieve scores that are 0.40 lower than that of white students (Model 4). However, including the three key covariates – (1) a proxy for the age of the mother, (2) a component measuring socioeconomic status, and (3) participation in WIC – not only erases the effect, actually has the effect of reversing it,

Table 1.7.1: Replication of Fryer and Levitt (2004).

	Mathematics				Reading			
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
(Intercept)	0.16*	0.08*	-0.29*	-4.59*	0.06*	-0.02*	-0.37*	-3.58*
	(0.01)	(0.01)	(0.02)	(0.14)	(0.01)	(0.01)	(0.03)	(0.15)
Black	-0.62*	-0.36*	-0.23*	-0.09*	-0.40*	-0.14*	-0.02	0.11*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Hispanic	-0.72*	-0.43*	-0.30*	-0.20*	-0.44*	-0.24*	-0.16*	-0.10*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Asian American	0.15*	0.06	0.18*	0.25*	0.32*	0.23*	0.34*	0.38*
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Other	-0.49*	-0.32*	-0.26*	-0.16*	-0.40*	-0.24*	-0.17*	-0.09*
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
SES Composite		0.45*	0.37*	0.30*		0.44*	0.38*	0.30*
		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
No. of Books			0.01*	0.01*			0.01*	0.01*
			(0.00)	(0.00)			(0.00)	(0.00)
No. of Books ²			-0.00*	-0.00*			-0.00*	-0.00*
			(0.00)	(0.00)			(0.00)	(0.00)
Female				-0.01				0.13*
				(0.02)				(0.02)
Age in the Fall				0.06*				0.04*
				(0.00)				(0.00)
Birth Weight				0.00*				0.00*
				(0.00)				(0.00)
Teenage Mother				-0.10*				-0.15*
				(0.02)				(0.02)
Mother 30 or Over				0.18*				0.20*
				(0.02)				(0.02)
WIC				-0.20*				-0.16*
				(0.02)				(0.02)
N	12323	12323	12192	11218	11694	11694	11569	10661
R ²	0.11	0.22	0.24	0.32	0.05	0.16	0.17	0.23
adj. R ²	0.11	0.22	0.24	0.32	0.05	0.16	0.17	0.23
Resid. sd	15.32	14.33	14.15	13.43	15.63	14.69	14.55	14.15

Standard errors in parentheses

* indicates significance at $p < 0.05$

resulting in a positive, statistically significant effect on the black status (Model 8), a surprising result.

Making a causal (or even quasi-causal) claim on the basis of only these results would

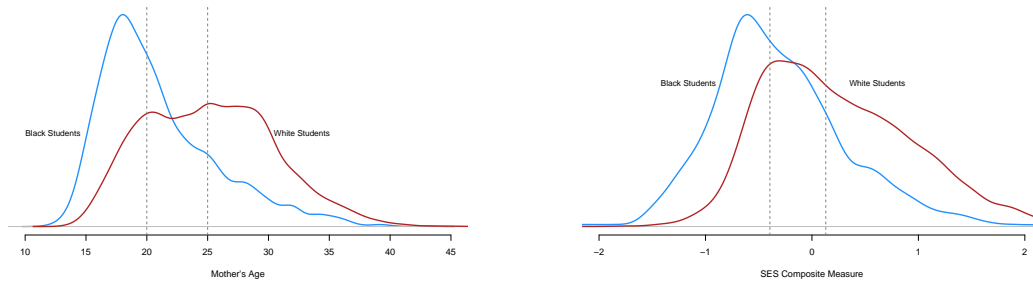


Figure 1.7.1: Distribution of Mothers' Ages (Left) and SES Composite Measure (Right), Disaggregated by Race

be misguided, and the methodology discussed here counsels against this sort of kitchen-sink econometric analysis. First, all of the key variables that erase the effect of the black variable are realized post-treatment, meaning that they are fundamentally driven and affected by a students' race. Second, and relatedly, all three of the key variables fluctuate substantially according to the students' race. For example, among these students, 76% of black kindergardeners come from families enrolled in WIC, but only 29% of white kindergardeners do. Similarly, black students are more likely to have had younger mothers and are more likely to have low SES measures (Figure 1.7.1). Thus, when understanding whether there is a causal effect of race on the educational achievement gap, controlling for these factors that vary substantially (and are affected) by race introduce bias into an estimate of the total effect of race. (In other words, if we control for some components affected by race and not others, what does the remaining coefficient on the race variable actually mean?) A better interpretation of Model 8 would be to take these (as Fryer and Levitt do) as providing intuition for a causal mechanism – i.e., the pathway via which race plays a role in this outcome.

To this extent, a more appropriate analysis (and what Fryer Jr and Levitt do later) is to

move forward with within-race comparisons. For example, consider WIC status. We could easily imagine an experiment where black children are born to families, and then these families are randomly sorted into WIC and not WIC-receiving families. We can similarly imagine other experiments for other components of the SES measure, or for the number of books a family own (another of the variables that Fryer and Levitt control for). Unlike the results presented in Model 8 (or Model 4), these kinds of research designs allow for identification (at least in theory) of a plausible, manipulable treatment(s). Thus, knowing that WIC, teenage mothers, and SES status substantially attenuate the naive “effect of race” gives us a valuable starting point; these are possible alternate treatments that we should explore via more rigorous analysis.

We illustrate some of these notions by presenting within-race analyses, focusing on the three leading suspects of WIC, teen moms, and low SES. For example, suppose that our working hypotheses is that black students are more likely to come from families with low SES measures, and that these SES measures are determinative of their relatively poorer performance. We thus have a treatment (low SES status), a moment of treatment (which is realized post-birth), and an identified hypothetical experiment. We also have well defined potential outcomes – a black student with low SES versus the same black student with high SES. To determine the relative role of SES (i.e., the average treatment effect of low SES), we use matching. Although simple regression would be adequate to tease out preliminary relationships, matching is preferable because it isolates this effect regardless of the possible ways that other variables may be affecting one another. To implement the matching, we use coarsened exact matching (Iacus, King and Porro, 2012). We match on those characteristics that would be considered pre-treatment, including the mother’s age

Table 1.7.2: Change in test scores, after matching, associated with (1) going from high to low SES scores, (2) having a teenage mother, and (3) coming from a family enrolled in WIC.

	Change in Outcome	95% CI	Matched <i>n</i>
Blacks			
Having Low SES	-0.49	(-0.58, -0.40)	903
Having Teen Mom	-0.24	(-0.34, -0.14)	1292
Enrolled in WIC	-0.36	(-0.45, -0.27)	1115

and the student's weight at birth. We do not match on those characteristics that would be affected by low SES status, such as the number of books owned in the home.

The results are presented in Table 1.7.2, and show a clear and convincing evidence that low SES adversely affects educational outcomes. Combined with the fact that black students are more likely to come from low SES backgrounds, this provides clues into the causal mechanism behind black students' performance. Similar results are found for having a teenage mother and for being enrolled in WIC. Thus, our conclusions dovetails with Fryer Jr and Levitt's: There is nothing inherent about race that drives the achievement gap. Rather, we can disaggregate factors commonly associated with one racial group and attempt to make causal inferences about these sort of characteristics.

1.8 EMPIRICAL EXAMPLE: WHO FIGHTS IN AFRICAN MILITIAS?

Another example is taken from Humphreys and Weinstein (2008), who use survey data to explore which personal characteristics will cause an individual to join civil war militia groups. Previous scholarship has suggested that individuals from marginalized groups have a higher incidence of joining militias. Looking at Sierra Leone's (1) opposition Revolutionary United Front (RUF) and (2) government-backed Civil Defense Forces

(CDF), Humphreys and Weinstein consider, among other hypotheses, whether members of the politically excluded Mende ethnic group are more likely to join militias.

To test this hypothesis, Humphreys and Weinstein include a Mende dummy variable (1 if Mende, 0 otherwise) in a logit regression that has membership in either militia group as the outcome variable and a host of additional variables as controls. Table 1.8.1 presents our replication of the Humphreys and Weinstein results, which is identical to theirs to the tenth decimal place and has the same number of observations. In analyzing more closely the role Mendes play, however, we should recognize that being Mende is considered in Sierra Leone an immutable characteristic, and it is therefore a “treatment” administered at birth. Accordingly – and acknowledged by Humphreys and Weinstein – an individual’s Mende status is causally prior to a host of other variables included in the model; the inclusion of these other variables (e.g., whether respondent lives in a mud hut) therefore introduces post-treatment bias into the estimate of the total effect of being Mende. Results after removing post-treatment variables are presented in Table 1.8.2.

For the RUF membership (the opposition militia), the significance of the Mende variable does not change – self-identifying as Mende is related positively with membership in the RUF, although the size of the effect is reduced (a move that makes sense given the RUF’s status as a Temne-backed organization). The results are, however, different for the government-backed, Mende-supported CDF. The original model (Table 1.8.1) shows that the Mende status coefficient, although positive, is not statistically significant. Once we remove the variables directly affected by Mende status, we see that being Mende *is* positively linked with belonging to the CDF, and that the effect is significant when we control for age. Interestingly, the effect associated with Mende

self-identification is about as strong for membership in the RUF *and* the CDF, a finding makes more substantive sense. The CDF, after all, is thought to represent Mende interests.

To explore the difference between Mendes and non-Mendes further, we focus specifically on membership in the government-backed CDF. Table 1.8.3 shows the results of separate logit regressions on Mende and non-Mende populations, with membership in the Mende-backed CDF as the outcome variable. While we don't advocate this strategy in all instances, it does help to isolate the importance of different traits in the different groups – and to show the effect modification associated with the ethnicity variable. Indeed, what this regression shows is that different variables are important for the different groups (Table 1.8.3). For the Mende, living in mud housing (a proxy for poverty) is not a predictor of CDF membership, whereas for the non-Mende, it is. Likewise, having a friend in the CDF is not predictive, but it is for non-Mende. For the Mende group, it is being a boy or a man (as opposed to girl or woman) that is predictive of membership in the CDF.

Specifically, both Mende and non-Mende individuals are receptive via factors associated with economic grievances – poverty (mud housing) and less education are more likely to lead to membership in the CDF. On the other hand, the Mende people are less susceptible to selective incentives – e.g., friendship, and, to a lesser extent, money and safety. These results suggest that many Mendes already have a natural affinity for the Mende-backed CDF – not necessary through friendships, but through shared ethnicity. By contrast, for non-Mendes, for whom no pre-existing affinity exists for the CDF, monetary and social incentives are more salient. This inclination is borne out by a simple interacted model, represented in Table 1.8.4, although we note that the interaction term is

not significant.¹²

1.9 A UNIFIED FRAMEWORK FOR RACE AND CAUSALITY

In this paper, we have highlighted both the pitfalls and the possibilities associated with trying to extract meaningful causal inferences about race in a quantitative framework. Most quantitative social scientists try to gain leverage on the causal impact of race by including simple dummy variables, along a standard battery of control covariates. As we note in this article, however, race presents unique challenges for quantitative scholars. First, race is resistant to manipulation and, hence, potential outcomes are ill-defined and research questions fundamentally unidentified. Second, because race is “assigned” at birth, the host of characteristics that most social scientists control for (age, education, income, etc.) occur after the treatment is assigned and therefore potentially introduce bias into the estimate of interest. Third, an equally meaningful problem is that race is too complex to be synthesized into one neat variable. To the contrary, how a person is categorized by society or self-identifies is inextricably intertwined with tangible measures such as education, income, health, diet, economic status as well as intangible factors as culture, traditions, and political and social attitudes. Thus, the introduction of a race “dummy” variable – along with attendant background covariates – oftentimes does a disservice to queries that look to make causal inferences about race-based characteristics.

The framework described in this article may help researchers interested in race or ethnicity extract those kinds of inferences that capture a causal effect. First, we suggest that researchers interested in race begin by thinking whether their research design may be

¹²Mediation analyses using the `mediation` package were less conclusive.

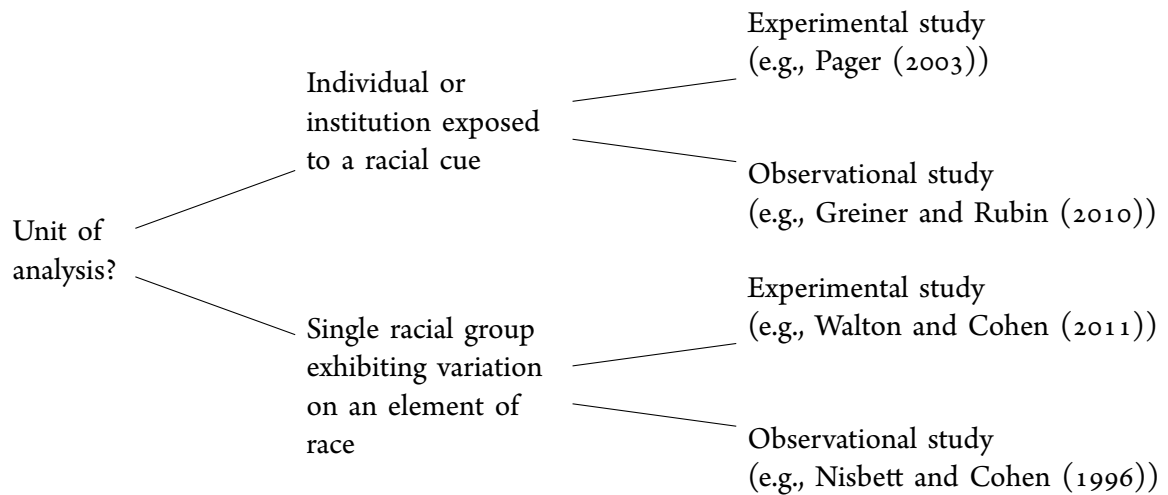


Figure 1.9.1: Appropriate Research Designs Assessing Race and Causality

appropriately captured by an exposure study. This kind of research design may be particularly appropriate for those studying public opinion, political behavior, law, and public policy – fields in which questions of interest frequently involve how institutions or individuals view and interact with racial signals and cues. Because the exposure research design avoids the pitfalls outlined above, it serves as an extraordinarily useful (yet underused) research design.

Second, when it comes to research designs focusing on minority populations themselves, researchers may actually be able to focus on some alternate manipulable treatment regime that varies closely (perhaps exclusively) with race. Here, we find the analogy to the “bundle of sticks” a useful one; and even though “biological race” itself may not be subject to manipulation, things like name, culture, neighborhood, dialect, and diet – i.e., those variables that define the contours of racial identification – may be experimentally manipulated and observationally assessed. We do not attempt to say that

such an alternate treatment may be found in all instances; rather, the takeaway is that (a) such an alternate treatment may vary closely with race, (b) may not already be included in the analysis, and (c) may explain away much of the effect previously attributed to race. Focusing on treatments other than the immutable aspects of the race of a subject not only solves problems with fundamentally unidentified research questions and ill-defined potential outcomes, but it also forces researchers to consider exactly what is being captured by the racial identification variable. Both of these are welcome considerations – both in terms of increasing statistical rigor and also in terms of increasing substantive engagement with developments in the racial and ethnic politics literature.

Three further issues are worth flagging. First, we suggest throughout that researchers think carefully about post-treatment bias issues. This is not a new warning (e.g., King, Keohane and Verba (1994)), but it carries particular urgency when it comes to race and causality. Race, which is assigned in part at birth, has immutable components, which means that the host of variables that social scientists routinely control for may be determined post-treatment and could therefore introduce bias into the causal estimate. To rectify this issue, researchers interested in the causal impact of race should think carefully and what is and what is not post-treatment. Our suggestions for exposure studies and within-race analyses substantially help in this regard. Second, we note that many possible alternate treatment regimes vary almost exclusively by race and, therefore, comparisons between whites and blacks, Hispanics and Asians, etc., may be of limited use due to problems with collinearity and a substantial (and persistent) lack of common support among key covariates. As a result, a useful way to explore whether alternate treatment regimes could be capturing some of the effects of “race” is to conduct

within-group comparisons. Finally, focusing on aspects of race that are substantive, mutable and identified can more plausibly be translated into meaningful policy interventions.

We conclude by emphasizing the importance of experimental analogies. This is a point that has been made by the causal inference and econometrics literature (Angrist and Pischke, 2009), but is particularly worthwhile for those specifically interested in race. Keeping an eye on what the ideal experiment would look like (and what factors would or would not have to be controlled for) is essential for thinking clearly about potential identification strategies and problems. Ultimately, keeping experimental analogies in mind – while also keeping in mind what precisely is measured with the inclusion of a “race” variable – will help scholars to reconcile race and causation.

Table 1.8.1: Logit Regression Replication of Humphreys and Weinstein (2008). Dependent variable is membership in the RUF or CDF militia groups.

	RUF Membership	CDF Membership
Intercept	-12.48*	-26.74*
	(3.17)	(3.58)
Mud Walls	0.92*	1.61*
	(0.41)	(0.56)
Lack of Access to Education	1.09*	0.80*
	(0.30)	(0.30)
Supports the SLPP	-0.49	-0.58
	(0.67)	(0.58)
Mende Ethnic	2.16*	0.58
	(0.87)	(0.65)
Does Not Support Any Party	1.29*	1.62*
	(0.57)	(0.50)
Offered Money to Join RUF	1.77*	
	(0.58)	
Felt Safer Inside RUF	-0.55	
	(0.37)	
Friend of RUF Members	0.24	
	(0.89)	
Villages Accessible by Foot or Boat Only	-0.01	0.03*
	(0.02)	(0.01)
Farmer	0.32	1.39*
	(0.56)	(0.46)
Student	0.83	1.26*
	(0.54)	(0.56)
Male	2.44*	4.06*
	(0.64)	(0.89)
Age	1.03	3.52*
	(1.20)	(1.23)
Age Squared	-0.20	-0.46*
	(0.16)	(0.15)
Freetown	-0.15	0.55
	(0.72)	(0.83)
Infant Mortality	13.52	16.85*
	(6.73)	(6.08)
Offered Money to Join CDF		3.19*
		(0.67)
Felt Safer Inside CDF		2.34*
		(0.30)
Friend of CDF Members		0.60
		(0.50)
N	59	64

Standard errors in parentheses

* indicates significance at $p < 0.05$

Table 1.8.2: Logit Regression Replication of Humphreys and Weinstein (2008). Dependent variable is membership in the RUF or CDF militia groups. Post-treatment variables have been removed from the model.

	RUF	RUF	CDF	CDF
(Intercept)	-6.27*	-3.49	-8.31*	-7.22*
	(0.75)	(1.87)	(0.84)	(1.65)
Mende	1.47*	2.08*	1.31	1.63*
	(0.70)	(0.73)	(0.69)	(0.71)
Gender	0.87*	1.53*	3.63*	3.92*
	(0.30)	(0.41)	(0.45)	(0.57)
Age		-1.46		-0.56
		(0.87)		(0.85)
Age Squared		0.09		0.03
		(0.10)		(0.09)
N	59	59	64	64

Standard errors in parentheses

* indicates significance at $p < 0.05$

Table 1.8.3: Comparing models fitted on the Mende population versus on the non-Mende population. Coefficients are logit estimates (standard errors in parentheses). Outcome variable is whether an individual joined the CDF or not.

	Mende Only	Non Mende Only
Intercept	−21.71* (3.73)	−25.25* (9.12)
Mud Walls	2.06* (0.61)	2.18* (0.83)
Lack of Access to Education	0.81 (0.52)	0.86* (0.29)
Supports the SLPP	0.00 (0.76)	−1.78 (1.05)
Does Not Support Any Party	1.75* (0.84)	2.03* (0.58)
Offered Money to Join CDF	3.33* (1.09)	3.66* (1.17)
Felt Safer Inside CDF	2.20* (0.43)	2.72* (0.37)
Friend of CDF Members	−0.82 (0.74)	2.09* (0.65)
Villages Accessible by Foot or Boat Only	0.03 (0.03)	0.05 (0.03)
Farmer	2.10* (0.76)	1.03* (0.50)
Student	1.03 (0.86)	0.38 (0.69)
Male	6.15* (1.03)	2.01* (0.61)
Age	0.41 (1.28)	1.46 (1.26)
Age Squared	−0.16 (0.15)	−0.15 (0.13)
Freetown	2.55 (1.35)	1.47 (1.48)
Infant Mortality	19.22* (6.22)	26.47 (41.49)
N	41	44

Standard errors in parentheses

* indicates significance at $p < 0.05$

Table 1.8.4: Simple interacted model. Logit coefficients. Outcome variable is membership in the CDF.

(Intercept)	−8.523*	−8.140*
	(0.850)	(1.601)
Mende	1.411	1.722*
	(0.740)	(0.780)
Male	3.474*	3.722*
	(0.435)	(0.527)
Friend of CDF Members	2.122*	2.383*
	(0.652)	(0.652)
Mende:Friend of CDF Members	−0.708	−1.133
	(0.992)	(1.025)
Age		−0.134
		(0.799)
Age Squared		−0.024
		(0.089)
AIC	22.771	26.713
BIC	65.949	87.162
log L	8.614	14.644

Standard errors in parentheses

* indicates significance at $p < 0.05$

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2

Violence and Voting

DID THE 1960S URBAN UPRISINGS RESHAPE AMERICAN POLITICS?

2.1 INTRODUCTION

In 1964, Barry Goldwater campaigned for the presidency promising “law and order” against “crime in the streets.” Though Goldwater lost in a landslide, two years later Ronald Reagan closely echoed Goldwater’s rhetoric and won the California Gubernatorial race in a blowout. Why was law and order a losing campaign strategy for Goldwater in 1964 yet a winning strategy for Reagan in 1966? Similarly, how was it that Goldwater’s racially tinged anti-crime rhetoric gained little traction outside of the deep south, yet Reagan’s invocation of law and order found broad appeal in relatively progressive California? A number of scholarly and journalistic accounts suggest that violent protests in the summer of 1965 — particularly unrest in the Watts neighborhood of Los Angeles — transformed California politics, increased concern about order, and drove white moderates from the Democratic to the Republican party.

This “backlash” account has also been used to explain the increased salience of law and order in national elections following Reagan’s 1966 Gubernatorial victory. While extrapolating from the single case of California would offer little insight into the systematic effects of violent protests on electoral behavior, by 1968, more than 500 other black-led violent protests had erupted across the country. In his 1968 run, Republican candidate Richard Nixon successfully marshaled tough-on-crime oratory to gain a commanding lead on the issue. In the 1980s and 1990s, candidates including Reagan, George H.W. Bush and Bill Clinton also effectively deployed tough-on-crime campaign strategies to help them carry the highest office in the land. The success of these law and order strategies profoundly reshaped American politics and criminal justice policy in the

United States (Gottschalk, 2006; Alexander, 2010). Yet, why law and order emerged as a salient political issue in the mid-to-late 1960s remains a subject of scholarly debate (Weaver, 2007; Murakawa, 2008). In particular, how violent protests between 1964 and 1972 might have influenced the rise of law and order in American politics remains unclear.

For most of American history, race riots were initiated by white mobs that fomented violence against blacks. Outbreaks of mayhem like the New York City Draft Riots of 1863 could result in dozens of blacks losing their lives and hundreds having their property stolen (Bernstein, 1991). Prior to the 1960s, black-initiated riots were rare. Beginning around 1964, however, the number of spontaneous, black-initiated violent protests grew dramatically through 1968, and then declined almost as quickly through the early 1970s.¹ While the median urban riot involved no deaths, one incident of arson and 10 arrests, a handful of the clashes between protestors, residents, rioters, local businesses and law enforcement resulted in dozens of deaths, hundreds of arson incidents, and thousands of arrests. The most serious riots were front page news across the country.

Scholars of American politics have tended to focus on how riots shaped the behavior of elite actors and legislative outcomes. Their findings point to increased efforts at redistributive policies and also, possibly, increased investment in repressive capacity (Fording, 2001; Giugni, 1998). This literature generally offers little insight into how riots may have influenced the voting behavior of the mass electorate, however. An interdisciplinary literature on the rise of “law and order” campaigns and policies debates whether rising crime and disorder led to a “backlash” among voters. Flamm (2005)

¹Throughout this article I use a variety of phrases—such as political violence, violent protests, black insurgency, ethnic violence, ethnic riots and urban riots—interchangeably. I do this in keeping with the range of terminology commonly used to describe these events in scholarly writing across sociology, economics and political science over the last four decades.

presents a detailed historical account of how rising crime and social disorder evolved into a popular call for tougher criminal justice policy. Other scholars question the backlash argument and suggest that forces such as elite campaign strategies (Weaver, 2007) and immigration (Beckett and Godoy, 2008) have been the more relevant issues driving voting behavior.

To date, the debate over whether violent protests helped fuel a backlash among a significant swath of voters has lacked systematic quantitative support. The arguments in favor of a backlash have also lacked a well articulated theory of when and why it occurs in some instances but not others (Weaver, 2007). To address the empirical gap, I use data on voting, public opinion and urban riots to investigate if the black political violence of 1960s and 1970s influenced mass electorate voting behavior. I also offer a theory of perceived existential threat, extending the literature of racial threat, to suggest how violent protests might directly affect mass voting behavior.

I calculate each counties' unique exposure to violent protests with a novel composite measure that, for every county-riot dyad, accounts for distance, timing to a specific election and riot intensity. All things being equal, violent protests that are geographically closer to a county, temporally closer to an election and exhibit greater intensity (as measured in the number of arrests) will increase the riot 'treatment' for that county. Examining county-level voting patterns, I find that in the 1968 presidential election exposure to violent protests is strongly and negatively associated with Democratic vote share. I find a similar negative relationship between exposure to violent protests and Democratic vote share in congressional elections between 1966 and 1972. Examining counterfactual scenarios, I estimate that fewer violent protests are associated with a

substantially increased likelihood that the Democratic presidential nominee, Hubert Humphrey, would have beaten the Republican nominee, Richard Nixon, in the 1968 election. As African Americans were strongly identified with the Democratic party in this time period, my results suggest that, in at least some contexts, political violence may contribute to a backlash among segments of the mass electorate and encourage outcomes directly at odds with the preferences of the protestors.

This paper proceeds as follows: the second section provides background information on the 1960s unrest, including a discussion of the California case, as well as descriptive statistics on voters' racial anxieties and desire for "social control." The third section surveys the relationship between violence and voting in the literature and presents a theory of why racialized violence led to a backlash in the American case. I discuss my data, models and results in Sections 4, 5 and 6. In Section 7, I discuss possible extensions and, finally, offer concluding thoughts in Section 8.

2.2 THE LONG, HOT SUMMERS

On an unusually warm evening in August 1965 in Watts, a predominantly black neighborhood of Los Angeles, Lee Minikus, a white California highway patrolman pulled over Marquette Frye, a black resident, for driving erratically. After Frye failed to pass a sobriety test, Minikus arrested Frye and arranged for Frye's car to be impounded. When Frye's mother and step-brother attempted to intervene, the seemingly routine traffic incident escalated and Minikus arrested and handcuffed all three. As Minikus took the Frye family to a police station, an increasingly incensed crowd of observers began protesting the perceived injustice by throwing stones at passing cars, particularly patrol

cars. Amid a poorly coordinated police response, rumors of police aggression against a pregnant woman circulated widely and further inflamed the crowd. The next day local black leaders attempted to calm the situation but, by nightfall, a new wave violent protest began that did not end for five more days. After six total days of protests and riots, 34 people had been killed, 3,000 incidents of arson recorded and nearly 4,000 people arrested.

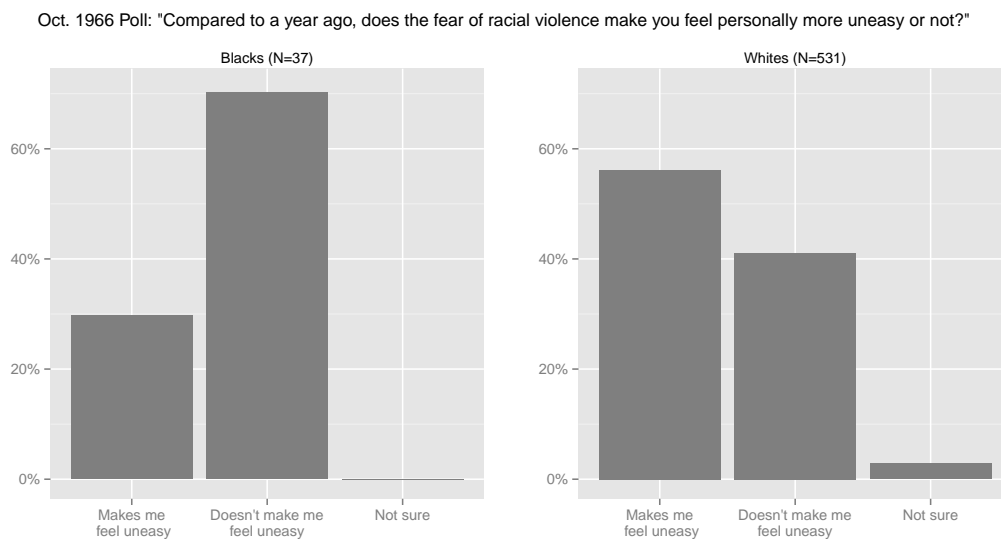


Figure 2.2.1: Bar plot of public opinion on the fear of racial violence in California, by race. Sources: Louis Harris and Associates, Inc. (1966) in California.

The Watts uprising transformed South Central Los Angeles and California politics. Incumbent Governor of California Pat Brown had a 42 percent approval rating in January of 1965, but with a long list of legislative accomplishments and a booming economy, seemed well positioned for reelection (Our Campaigns, 2011; Flamm, 2005). Following the Watts uprising and anti-war protests at Berkeley, however, his campaign began to

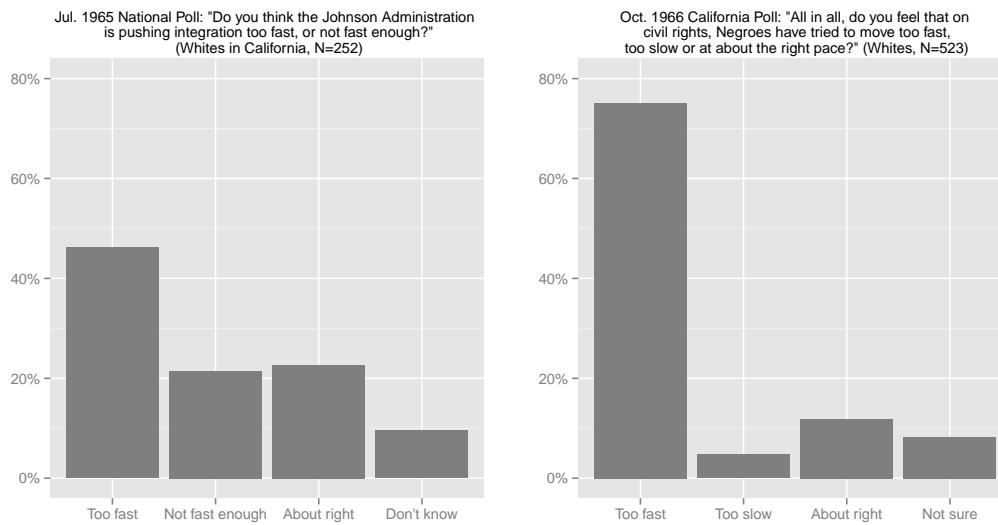


Figure 2.2.2: Bar plot of public opinion on civil rights among whites in California in 1965 and 1966. Sources: Left panel from The Gallup Organization (1965). Right panel from Louis Harris and Associates, Inc. (1966) in California.

unravel. In the Democratic primary for governor, Los Angeles mayor Sam Yorty challenged the incumbent and captured a surprising 38 percent of the vote by courting working-class, white Democrats who abandoned Brown en masse (Flamm, 2005, 70). Yorty waged an aggressive campaign that promised to restore “law and order.” It was a template the Republican candidate for Governor, Ronald Reagan, emulated and, in the November election, Reagan crushed Brown, taking nearly 58 percent of the vote and winning by a one million vote margin.

Public opinion polls conducted in California during the run-up to the election offer some tangible insight into the level of racial anxiety among white voters after Watts. The right panel of Figure 2.2.1 presents the responses of whites to a Louis Harris and Associates, Inc. (1966) poll question from October, 1966. When asked, “Compared to a

year ago, does the fear of racial violence make you feel personally more uneasy or not?” 56 percent of white Californians responded affirmatively while 41 percent indicated that they did not feel more uneasy as compared with a year ago. By contrast, though almost all of the violence initiated by protestors and police took place within black neighborhoods, as seen in the left panel Figure 2.2.1, only about 30 percent of black Californians felt uneasy and 70 percent did not feel uneasy.

Figure 2.2.2 presents two bar plots of public opinion polls on the question of whether the push for integration and civil rights was moving “too fast.” In the left panel, which was conducted with a national sample prior to the Watts uprising in August of 1965, 43 percent of respondents agree that the Johnson Administration is pushing too fast on integration. The right panel, conducted within California after Watts, presents responses to the question “All in all, do you feel that on civil rights, Negroes have tried to move too fast, too slow or at about the right pace?” Among white respondents, 75 percent chose “Too fast,” 5 percent chose “Too slow,” and 12 percent responded “About right.”² Though the wording of the questions differs, the 30 percentage point increase over a little more than year is at least suggestive that the violent protests in Watts may have contributed substantially to anxiety about civil rights among whites in California.³

Prior to Watts, black-led violent protests were relatively rare. As can be seen in Figure 2.2.3, the number of urban riots grew dramatically from 1965 through 1968, the year of Martin Luther King, Jr.’s assassination, and then declined almost as quickly. A

²In contrast to whites, 26 percent of blacks responded “Too fast,” 26 percent responded “Too slow” and 42 percent responded “About right.” (Louis Harris and Associates, Inc., 1966).

³The only other major civil rights related event in that period was the enactment of the Voting Rights Act (VRA) in August of 1965. It seems implausible that the VRA could have moved white opinion in California so dramatically given that blacks were already enfranchised in California and the VRA did not apply in the state.

growing body of evidence suggests that riots can spread in a contagion-like manner (Olzak, 1994; Myers, 2000). It is possible that the heavy media coverage accorded to the Watts riot played an important role in diffusing the idea of violent protest across the United States, and following King's assassination, at pushing the number to a peak.

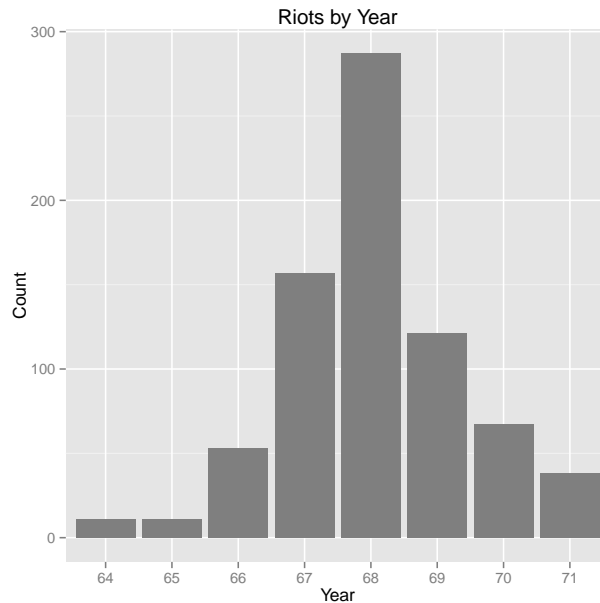


Figure 2.2.3: Bar plot of riots by year, 1964 to 1971. Sources: Carter (2005)

Whether or not the Watts uprising served as a critical turning point, urban riot activity increased markedly after 1965. The spike and decline in riot activity corresponds to a similar rise and fall in public concern about “social control.” Figure 2.2.4 presents data on what Americans, when surveyed, indicated was the “most important problem” facing the country between 1950 and 1980. In Figure 2.2.4, the percentage of Americans listing “social control” as the most important problem remained in the single digits until the mid-1960s when it spiked and reached an initial peak of about 41 percent in August of

1967 before declining rapidly after 1971.

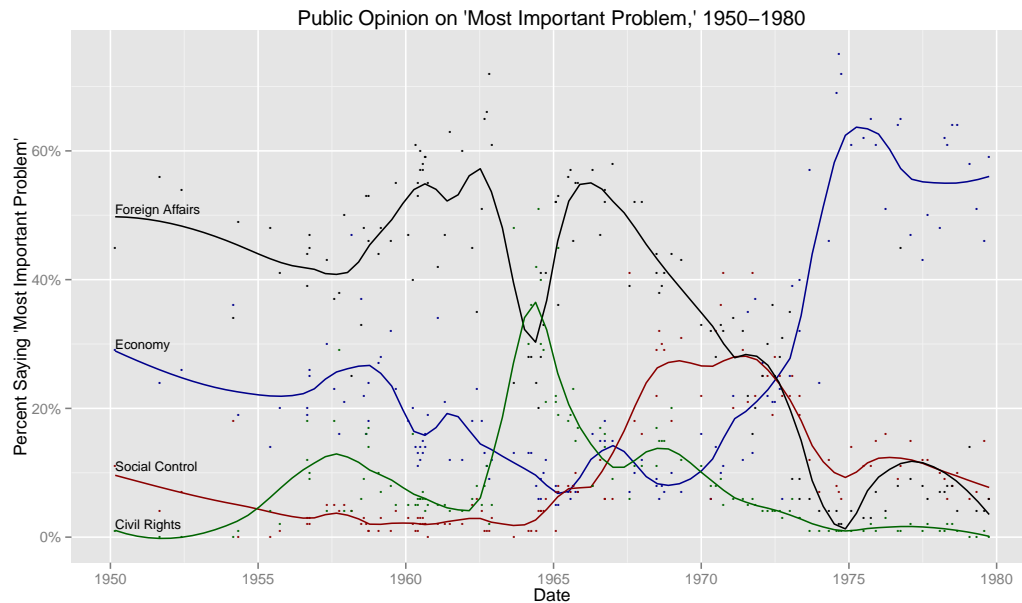


Figure 2.2.4: Scatter plot of public opinion on the ‘Most Important Problem,’ 1950 to 1980. Source: Loo and Grimes (2004); Niemi, Mueller and Smith (1989). (Lines smoothed with Loess curve.)

A plot of riot and public opinion data shows a strong correlation between riots and demand for law and order at the national level. Figure 2.2.5 presents a plot of riot severity for 752 riot events by month and year using data from Carter (2005) and public opinion data from from Niemi, Mueller and Smith (1989). The logged number of arrests is presented on the left-hand y-axis. The public opinion data scale is presented on the right-hand y-axis and details what percent of those surveyed identified “social control” as

the most important problem facing America.^{4 5} The measures not only follow similar year-over-year patterns, but also season-by-season.

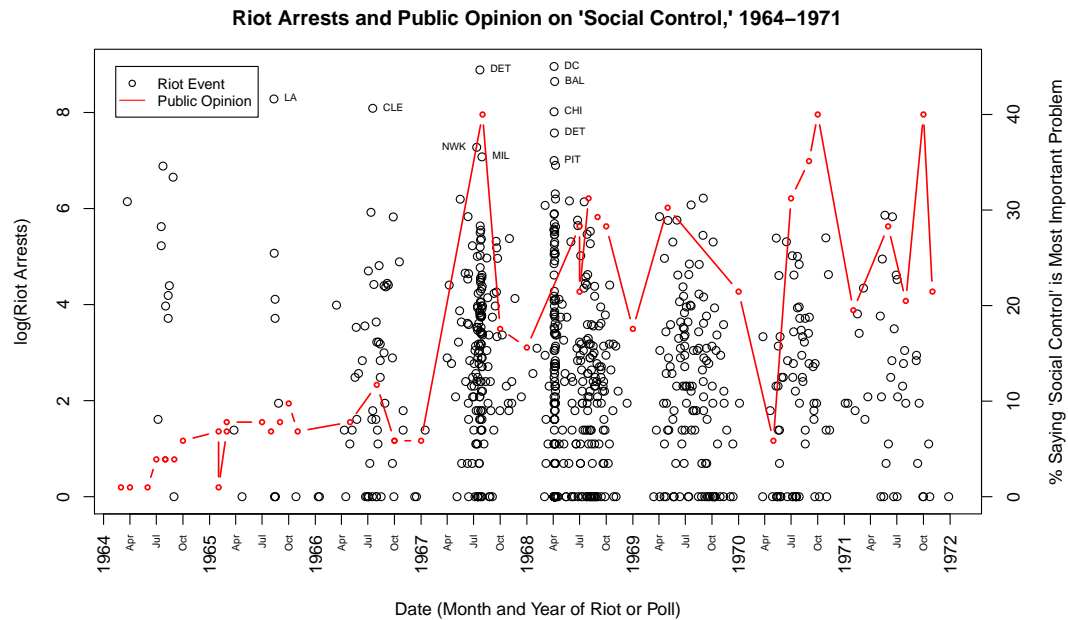


Figure 2.2.5: Scatter plot of logged riot arrests (left y-axis) and public opinion on 'social control' (right y-axis), 1964 to 1971. Sources: Niemi, Mueller and Smith (1989); Carter (2005). The ten riots in which more than 1,000 people were arrested are labeled with abbreviated city names.

As evidenced in Figure 2.2.5 the Watts uprising (labeled LA in the plot, above August, 1965) was substantially different from any other urban riot in 1964 or 1965 (especially when considering that the scale is logarithmic and does not account for the record levels

⁴The "social control" measure is a composite of several different categories including concern about crime, civil unrest, communist agitators, juvenile delinquency and other issues. The polling data is also sourced from more than one pollster. For a critique of this composite measure, see Loo and Grimes (2004). Evidence suggests that many citizens conflated issues like violent protests and communist agitation so I use the composite measure without adjustment (Flamm, 2005).

⁵The Niemi, Mueller and Smith (1989) public opinion data is the same as in Figure 2.2.4 except that I only present the percentage of people who say "social control" is the most important problem and exclude the measures for civil rights, foreign policy and the economy.

of arson that occurred). According to the data presented in Figure 2.2.5, national public concern about “social control” only spikes modestly following the Watts riot but does generally exhibit cumulatively higher peaks and valleys with each successive wave of unrest through the early 1970s.

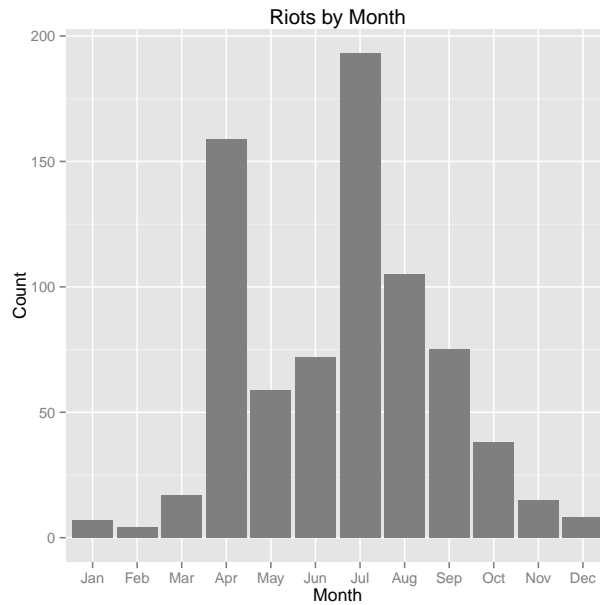


Figure 2.2.6: Bar plot of urban uprisings by month. Sources: Carter (2005).

More significantly, the public opinion data exhibits significant variation within year. Consistent across almost every year is a peak around mid-year and a trough around the new year. Figure 2.2.6 presents the frequency of 1960s and 1970s urban riots by month. As can be seen in Figure 2.2.6 riots are substantially more common in the summer months and less common in the winter months. The one exceptional month, April, is driven by the large number of riots that occurred after the assassination of Martin Luther

King, Jr. in 1968.⁶

Additional evidence from other historical and public opinion sources suggests widespread concern among the mass public about crime and disorder. Weaver (2007) notes that Members of Congress were deluged with “torrents of constituent mail” in favor of the 1968 Safe Streets bill and that even liberal Democrats “felt compelled by public anxiety over crime and riots to vote for the bill” (p. 257). Figure 2.2.7 presents polling data from August, 1968 in which 81 percent of respondents agreed with the statement “Law and order has broken down in this country” (Louis Harris and Associates, Inc., 1968a).

In the 1968 presidential election, Nixon and Humphrey polled at similar levels on many issues. On law and order, however, a wide gulf existed in public perceptions of the two candidates. Figure 2.2.8 presents the results of a poll that asked “Which presidential candidate do you feel could do the best job in handling law and order?” Not only did respondents favor Nixon over Humphrey by 36 to 23 percent, but third party candidate George Wallace also beat Humphrey in the poll by 26 to 23 percent. Though these polls offer no insight into the source of the anxiety, the results do suggest that such anxieties

⁶The synchronous seasonal variation in riot activity and national public concern about “social control” does not rule out the possibility that the correspondence is driven by factors other than civil unrest, such as seasonal variation in crime. One possible argument against seasonal confounders, such as crime, is visible in the variation in public opinion for “social control.” Across the eight years of data presented in Figure 2.2.5, public opinion is generally single peaked, typically around July. In 1968 and 1971, however, public opinion is double peaked with a rise and decline followed by a second rise and fall. Any theory of the seasonal concern for “social control” ought to be able to explain both the typical years as well as the atypical years. This double spike in 1968 might plausibly be explained by conflicting feelings of anxiety and sympathy for the violent protests that followed the assassination of Martin Luther King, Jr.. In 1971, the second peak in public opinion occurs in October, between lows in August and November. The October spike is plausibly explained by the Attica prison uprising which took place on September 9th, 1971. The violent protest data used in this paper excludes all riots occurring within institutions such as colleges and prisons. As such, Attica is not included in any of the data analysis or plots.

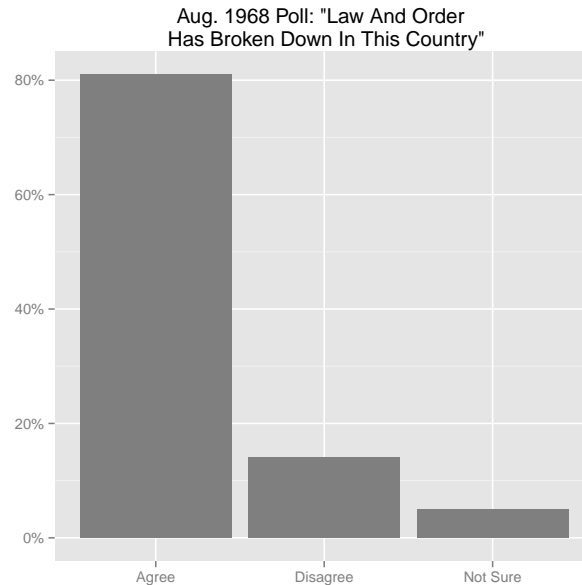


Figure 2.2.7: Bar plot of public opinion on “law and order.” Sources: Louis Harris and Associates, Inc. (1968a). Respondents were asked “(Now I want to read you some statements about law and order in this country. For each, tell me if you tend to agree or disagree)... Law and order has broken down in this country.”

were pervasive and worked very much to Nixon’s benefit.

2.3 COULD URBAN UPRISINGS HAVE SWAYED VOTERS?

How might ethnic violence influence voting in America? Figure 2.3.1 presents three possible models of how urban riots might be associated with Democratic vote share. Under the positive model, an increase in urban riots leads to increased Democratic vote share. While few American urban riots were associated with an explicit political agenda or party, following the Civil Rights Act of 1964, Democrats became the de facto party of

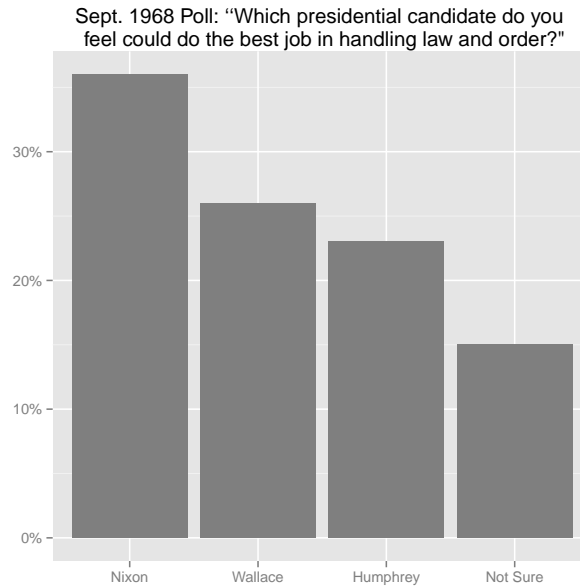


Figure 2.2.8: Bar plot of public opinion on presidential candidates and law and order. Sources: Louis Harris and Associates, Inc. (1968b).

African American voters (Carmines and Stimson, 1990; Bositis, 2008).⁷ As such, Democrats are assumed to be the party most aligned with black interests and preferences. A positive effect might occur if blacks voted in higher numbers when exposed to greater riot activity or if whites within the party felt sympathetic to the concerns of blacks and voted in higher numbers for Democratic candidates.

With the negative model, increasing levels of rioting by blacks are associated with declining Democratic vote share. The California Gubernatorial race might be an example of the backlash model in action if exposure to riot activity was associated with decreased

⁷According to data published by Bositis (2008, Table 1, p 8), between 1944 and 1960 black party identification with Democrats averaged about 53 percent. Between 1964 and 2004, black party identification with Democrats averaged 83 percent (See Figure 2.9.2).

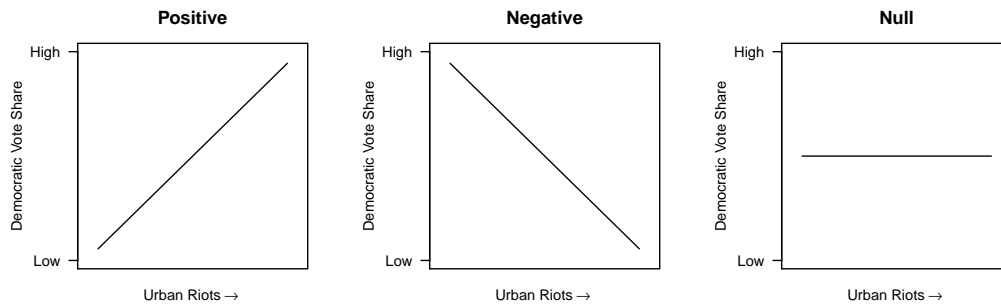


Figure 2.3.1: Hypothetical models of the relationship between urban riots and Democratic vote share.

support for Democrats by a significant number of whites. With a null model, the ethnic riots have no impact on voting behavior. The null model could describe a situation in which riots occur but are either less visible due to some other set of issues, say the Vietnam war, or make no lasting impression on voters below some threshold of activity. Other models that combine elements of these three are also possible.⁸

It is not immediately clear, based on the current literature, which model might be applicable to the case of urban riots in the U.S. The following sections survey the literature on ethnic riots and voting in the field of comparative politics, and on the 1960s urban riots in the field of American politics. The American case is an important, often missing part of the conversation in comparative politics. Similarly, the focus in American politics on elite decision-making during the 1960s urban riots misses the mass consequences of political violence on democratic politics, shown to great effect elsewhere in the world.

⁸These three proposed models are not meant to be exhaustive or definitive. For example, the true relationship between ethnic violence and Democratic vote share might exhibit some non-linearities in which small riots exert little or no effect and large incidents of violent protest exert a disproportionate effect.

2.3.1 ETHNIC RIOTS AND ELECTIONS IN COMPARATIVE PERSPECTIVE

Wilkinson (2004) offers a comprehensive analysis of the interplay between elections and ethnic riots. Using data from India, Wilkinson investigates what factors contribute to the occurrence of a riot or, conversely, work to suppress inter-ethnic violence. He finds that, at the local level, politicians often work to incite riots in an effort to increase the salience of ethnic ties and reduce the pull of other cross-cutting political cleavages like party affiliation, particularly as elections loom. In contrast to local political actors working to gin-up inter-ethnic rivalries, Wilkinson finds that Indian state-level politicians sometimes work to thwart budding riots while, at other times, facilitate such violence. He explains the variation with an electoral-incentives model that suggests state-level actors attempt to maintain the peace when such actions are useful to sustaining winning political coalitions.

In addition to finding supportive evidence for the electoral-incentives model in India, Wilkinson also applies the model to other cases, including the United States. While the white-on-black race riots of the American case fit the electoral-incentives model between Reconstruction and the 1950s, Wilkinson (2004) does not address how the electoral-incentives model would apply to the urban riots of the 1960s and 1970s. The urban riots that occurred between 1964 and 1971 did not generally manifest in significant inter-ethnic or anti-minority violence (except, perhaps, by blacks against white-owned property and by white law enforcement and National Guardsmen against blacks in their efforts to quell the riots). Rather, most of the violence was confined to black neighborhoods. Second, the electoral-incentives model does not clearly extend to the post-1960 riots. There is no evidence of which I am aware of local politicians in the U.S. strategically inciting riots for electoral gain, and no pattern of state or national actors

intentionally facilitating riots once underway. As such, the riots of the post-1960 period appear to be categorically different from the sort of Hindu-Muslim (or white-on-black) riots for which the electoral incentives model works well. In keeping with the electoral-incentives framework, however, despite the qualitative differences, these black-led violent protests might nevertheless have increased the salience of racial or ethnic cleavages in voting behavior.

2.3.2 RIOTS, REDISTRIBUTION AND REPRESSION IN AMERICAN POLITICS

In American politics, the dominant approach to understanding political responses to civil unrest has been to research whether elite political actors utilize positive or negative incentives to exert control. In the first case, elite state actors enact redistributive policies to temper insurgent voices. For example, in response to the popular unrest of the “Arab Spring,” one analyst estimated that Middle Eastern Gulf Coast governments have increased spending on social programs by \$150 billion (Kapur, 2011). Similarly, in the wake of the 1960s and 1970s urban riots, scholars have found that political elites responded to the civil unrest through increased spending on welfare programs like Aid to Families with Dependent Children (AFDC) and other redistributive policies (Fording, 2001, 1997; Skrentny, 1996; Hicks and Swank, 1983; Isaac and Kelly, 1981; Cloward and Piven, 1971).

In addition to “carrots,” states also mobilize “sticks” in which efforts at repression may trump or complement those of redistribution (Button, 1978). While much of the literature recognizes that state actors may respond with both redistributive and repressive policies, most research has focused on beneficent responses. Only a few scholars have

investigated whether civil disorders were associated with enhanced expenditures on policing and efforts at coercive control (Yates and Fording, 2005; Fording, 2001; Welch, 1975; Button, 1978; Feagin and Hahn, 1973; Iris, 1983). The results of these studies have been ambiguous, with some finding a positive relationship between political violence and state repression while others report null findings.

Results across the broader literature on the political consequences of violent protests and social movements have also been ambiguous. Summarizing the field, Giugni (1998) notes, "...in the whole it is difficult out of this impressive amount of empirical work to provide a clear-cut answer to the question whether disruption can produce policy changes ...". A number of methodological and conceptual problems likely contribute to the muddle in these results. First, prior work focuses almost exclusively on elite response as the outcome of interest. As a result, the studies tend to have a small number of units, many of which are likely influenced by idiosyncratic factors like historical and institutional constraints on elite decision making. Second, prior studies typically ignore geographic data beyond narrowly paired riot events and the immediately proximate governmental or administrative units (i.e. metropolitan statistical areas or states). Consequently, the aggregate effect of multiple riots across regions or even the nation is unobserved. Third, analyses of limited subsets of cities and riots may confound the results by biasing the samples.

2.3.3 RIOTS AND THE RISE OF PUNITIVE POLITICS

A third literature that investigates the effects of riots on voting is primarily concerned with the origins of law and order campaign strategies and policies. Theories that attempt

to explain the rising salience of crime in the public consciousness include a response to growing numbers of migrants and immigrants (Beckett and Godoy, 2008), a need absorb “surplus labor” (Wacquant, 2002), a product of the “prison-industrial complex” (Schlosser, 1998), successive waves of anti-crime campaigns (Gottschalk, 2006), the rise of the war on drugs (Bobo and Thompson, 2010; Miron, 2004), post-war social, ideological and technological transformations in “late modern” societies (Garland, 2001), the need to build winning conservative governing coalitions (Simon, 2007), and morality politics (Meier, 1994).

While each of these works offers insight into the rise of punitive politics, most fail to offer a convincing argument about timing. “Law and order” rhetoric has a long history in the United States (Finkelman, 1992; Murakawa, 2008) and so any successful theory of law and order politics should explain the dramatic rise in salience of social control in the 1960s and early 1970s. The theories of Wacquant (2002), Schlosser (1998), Gottschalk (2006), Simon (2007), Garland (2001) and Meier (1994) are too general to satisfy this concern and cannot adequately explain why the law and order campaigns arose nationally in the mid- to late-1960s rather than in the years before or after. Similarly, analyses like those of Bobo and Thompson (2010) and Miron (2004) offer persuasive arguments that drug prohibition has contributed significantly to increased incarceration but offer little insight into why drug prohibition or other tough-on-crime programs initially became popular with voters and legislators.

Beckett and Godoy (2008) offer a cross-national perspective of “hyper-penality” across the Americas. Narrowly, Beckett and Godoy cite a range of scholarship that suggests internal migration and immigration fuels fear and anxiety by natives and locals

who, in turn, support punitive responses to crime. In the U.S., internal migration was occurring for half-a-century before punitive politics become popular. Between 1940 and 1970, in the Second Great Migration, approximately five million blacks moved from the South to the North and West (see Figure 2.3.2). Despite this massive demographic shift, public opinion data on the concern for “social control” suggests it remained largely a non-issue until the mid-1960s and then increased just as overall black migration flatlined. Consequently, while migration may have been a necessary component of the rising desire for law and order it does not appear to be have been sufficient on its own.

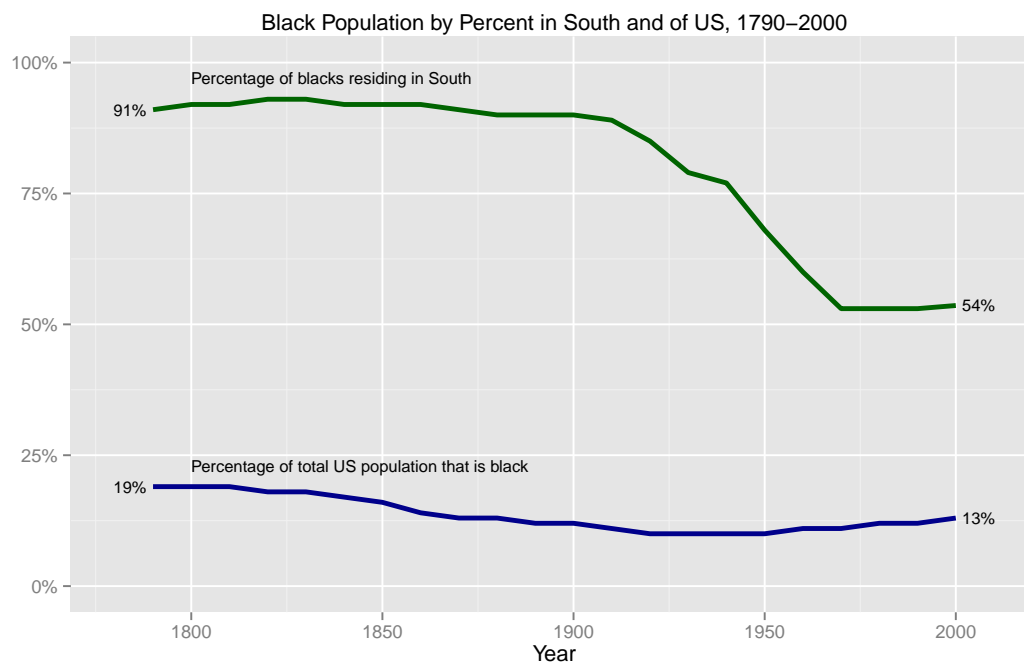


Figure 2.3.2: Line plot of black population by percent in South and of U.S. Source: U.S. Census Bureau, Gibson and Jung (2002)

Broadly speaking, two theories in the literature on the rising salience of crime in the public consciousness are attentive to the issue of timing. An older “backlash” hypothesis

argues that white masses mobilized against the perceived excesses of liberalism, particularly in response to crime and riots (Flamm, 2005). Flamm offers an historical account of the emergence of law and order politics beginning with growing concerns about juvenile delinquency in the late 1950s. Flamm argues that rising rates of crime and civil unrest in the 1960s were the key spark motivating changes in voter sentiment. Weaver (2007), by contrast, rejects this mass mobilization or “backlash narrative” in favor of a theory of “frontlash” or an elite-led push that caused citizens to be concerned with crime. In line with scholars of public opinion like Zaller (1992), the frontlash theory argues that agenda setting by elites was the critical factor driving mass opinion. With frontlash, Weaver contends that resistance to the civil rights movement drove opponents to seek new policy domains in which to compete. Responding to the success of the civil rights agenda, right-of-center elite political actors began to champion a new agenda built around law and order. Where Flamm emphasizes that law and order arose in response to growing levels of crime and social disorder, Weaver situates the taste for punitive policy in the broader racial struggles of the period. Thus, according to Weaver, the timing of law and order campaigns is essentially a reaction to the civil rights movement. Weaver comments that the strongest interpretation is, “punitive criminal justice was part of the price of civil rights liberalizations” (p. 265).

Both the backlash and the frontlash hypotheses leave important questions unanswered. Weaver argues the backlash analysis is more of a narrative than a theory and suggests it offers little guidance as to what circumstances should generate a backlash and what sorts of electoral or policy outcomes should be expected (Weaver, 2007, 237). Though the frontlash account is more fully theorized, the evidence in support of it is

ambiguous. Weaver writes “[t]wo crises served to elevate the status of violence as a political issue: crime and riots” (p. 244) yet also argues that “crime is not the primary explanation” for “*why crime came to be politicized in the 1960s and not before*” (p. 234).

The frontlash hypothesis also emphasizes the important role of the 1964 Republican presidential nominee Barry Goldwater’s nomination speech and campaign in triggering concern about crime in the general public (Weaver, 2007, 264). Weaver’s own data, however, suggest that the first spike in crime salience came in 1966, about two years after Goldwater’s 1964 campaign and after a modest dip in part of 1965. The public opinion data in Figure 2.2.5 suggests that until 1967, fewer than ten percent of the respondents mentioned factors related to “social control” (such as crime) as the most important problem in America. That concern about “social control” remained low in the immediate wake of the Goldwater campaign and then rose significantly before the Nixon campaign for the presidency began suggests that perhaps something other than elite political rhetoric was critical to the shift in public opinion. In addition, an elite-driven theory of public opinion would need to explain the seasonal spikes and troughs in public opinion on “social control” seen in Figure 2.2.5. The big spike in concern about “social control” in 1967, an off-election year, poses an additional puzzle for an elite-driven model.

An additional question of timing and geography remains for the the frontlash theory. Finkelman (1992, 2007) note that the argument that crime became increasingly racialized in the mid-1960s gives insufficient attention to the multi-century history of criminalizing black activity that was legal for whites. Given the long history, particularly in the deep south, of elected officials racializing crime and criminalizing black activity as an explicit campaign strategy, why did such efforts fail to succeed on the national stage

right through Goldwater's 1964 presidential election but then first become ascendent regionally in California in 1966 and nationally in 1968?

2.3.4 VIOLENCE, VOTING AND RACIAL THREAT

To address these and other concerns, I propose an extension to the literature on violence and voting that incorporates racial threat. Following Key (1949), a substantial literature has developed investigating the relationship between voting behavior and the presence of out-groups. Enos (2011) notes that despite the large literature on racial threat, little attention has been devoted to the mechanisms by which proximity to another racial group might motivate changes in voting behavior. To address this gap, he proposes a mechanism of "material threat" in which "individuals are responding rationally to competition over one or more outcomes, including economic success, political power, or social position" Enos (2011, 5).

In keeping with Enos' effort to more precisely specify mechanisms by which racial threat operates, I propose that "perceived existential threat" or fear of violence by an out-group may, at times, also be a key mechanism by which intergroup dynamics change voting patterns.⁹ Survey research in Los Angeles before and after the Watts riot found that African Americans became more black-identified following the unrest (Sears and McConahay, 1973). Similarly, the perceived existential threat mechanism suggests that, in response to fears about out-group violence, real or imagined, in-group and out-group cleavages become more pronounced and concerns about security become more salient.

⁹Whether perceived existential threat exists as a complement to or component of material threat depends in part on the definition of material threat. If by material competition Enos includes racialized fears of possible harm to one's body, family or property, then my conception of existential threat would be a subset of material threat.

In essence, the urban riots provoked many whites to become more white-identified and triggered a Hobbesian mindset in many white voters that prioritized in-group order and safety above other priorities. Wilkinson (2004) documents that repeated acts of mass violence associated with particular ethnic, racial or religious groups have the potential to increase the salience of in-group identities and out-group cleavages. In line with Wilkinson's findings, the perceived existential threat hypothesis suggests that the violence and black militancy associated with urban riots may have helped trigger a kind of white nationalism in some voters for whom, previously, white identity was not as salient.

In addition to raising the salience of white identity, the perceived existential threat hypothesis suggests that urban riots may have produced a disproportionate desire by whites for physical security and safety. A proxy for this anxiety can be found in data on gun sales. According to one estimate, "gun sales to whites more than doubled during the weekend after Watts" (Flamm, 2005, 59). Similar trends played out nationally. Farley (1980) reports that about 600,000 guns were sold, per year, in the first half of the 1960s but that more than two million guns were sold, per year, in the latter half of the 1960s and early 1970s. Citing a report from the National Commission on the Causes and Prevention of Violence (Newton, Zimring and on Firearms, 1969, Figure 11-5), Farley also points out, "in many cities a sharp increase in gun sales and registrations followed a riot. In Detroit, for example, the number of handgun permits issued by the police increased by a factor of five between 1965 and 1968."

The perceived existential threat theory also suggests under what conditions a polity might experience a "backlash" to different types of racial change and what downstream effects might occur in electoral and policy terms. In at least some cases, it is not racial

change, per se, that triggers an increase in the likelihood a significant majority of whites vote against the black-aligned party but, rather, racial change that occurs in concert with racialized mass violence. Thus, passage of the 1964 Civil Rights Act did not prevent Lyndon Johnson from winning the 1964 presidential election in a landslide and with the support of a majority of whites (the last Democrat presidential nominee to do so (Lublin, 1999)). Conversely, in the absence of widespread fears of racialized mass violence, Goldwater's two-fisted support for law and order did little to prevent him from losing badly.

Electurally, the perceived existential threat theory suggests that this combination of heightened racial identity and yearning for security will shift voting behavior in the group that perceives itself to be under attack away from the party affiliated with those committing the acts of collective ethnic violence. The group that perceives itself to be under attack will also shift towards parties that are strongly identified with the in-group. Put simply, urban riots helped raise the salience of whiteness in elections and increased demand for a white-identified party. This electoral outcome also helps explain why Democrats failed to capture law and order as a policy even though, for example, Lyndon Johnson helped launch the first national "war on crime." As long as Democrats were the party of blacks, they couldn't also be the party that would protect white voters who perceived an existential threat from blacks.

Policy-wise, the perceived existential threat hypothesis suggests that the "threatened" group will rally behind programs that promise security, especially promises of security couched in terms that reinforce the in-group and out-group cleavage. Playing to that anxiety, Nixon's 1968 presidential campaign cleverly married a liberal emphasis on rights

with conservative concerns about order. For example, during his nomination speech, Nixon proclaimed, “freedom from fear” is the first civil right of all Americans (Flamm, 2005, 177).¹⁰ Conversely, the existential threat mechanism also suggests that in times of relatively low collective ethnic violence, racial and ethnic bloc voting will be less likely and policies that promise order promotion will prove less salient. Thus, the relatively low rates of violence and especially mass violence seen in the United States in the last decade may have played a critical role in reducing a perception of existential threat and allowing a black candidate for president, Barack Obama, to build a winning cross-race coalition for his presidential election in 2008.

In sum, I suggest that a perceived threat of racialized violence is an important component of racial threat. Periods of significant racial and ethnic disorder are likely to increase racial and ethnic bloc voting and intensify popular support for repressive policies. While it is impossible to test the existential threat mechanism directly, I take voters’ exit from the Democratic Party as indirect evidence of an increased salience of race and preference for “law and order.”

To test the effect of riots on voters’ exit from the Democratic Party, I evaluate the relationship between political violence and electoral response using a measure of exposure to riots across 3,093 counties in the United States with county-level voting behavior as the outcome of interest. The new method for estimating the effect of riots calculates a unique riot ‘treatment’ for each county that incorporates spatial, temporal

¹⁰Currently, my specification of the perceived existential threat mechanism does not specify why campaign strategies that emphasize repression seem to be more effective at wooing voters than other sorts of anti-crime or anti-violence strategies. An initial hypothesis is that mass violence triggers fairly visceral reactions in some voters and that, once triggered, produce a bias for “rough justice” or vengeance over other sorts of policy interventions.

and riot intensity measures for every county-riot dyad in every election year between 1964 and 1972 (Formula 2.3 explains the riot measure in more detail). This unique riot ‘treatment,’ in turn, allows for the units of analysis to be every county in the continental United States. Finally, by using mass voting behavior in presidential and congressional elections as the outcome, the idiosyncratic aspects of unobserved elite decision making are largely avoided. Additionally, by analyzing mass rather than elite behavior, it may be possible to understand other mechanisms by which political violence drives policy change.

2.4 DATA

The primary data analysis in this paper estimates the relationship between voting behavior and exposure to urban riots. The outcome variable is the county-level percentage of votes going to the Democratic party in presidential and congressional elections between 1964 and 1972. The county-level voting data is drawn from Clubb, Flanigan and Zingale (1986) and the electoral vote data is taken from Leip (2005). As a control, the baseline county tendency to vote Democratic in elections is calculated from the mean Democratic vote share in the twelve years prior to 1964 (i.e. four presidential or seven congressional elections).

The explanatory variable measuring exposure to riots is built from data on 752 urban riots that occurred between 1964 and 1971. Carter (2005) defines a riot as an event that involves at least 30 participants and generates a detectable level of injury or property damage. The riot data from Carter (2005) provides the date, city and state of each disturbance as well as several measures of the riot’s severity such as the number of arrests,

injuries and deaths. As evidenced in Figure 2.4.1, a map of the locations of riot clusters, the phenomenon was national in scope with the exception of the inland region between the Pacific coast and the midwest.

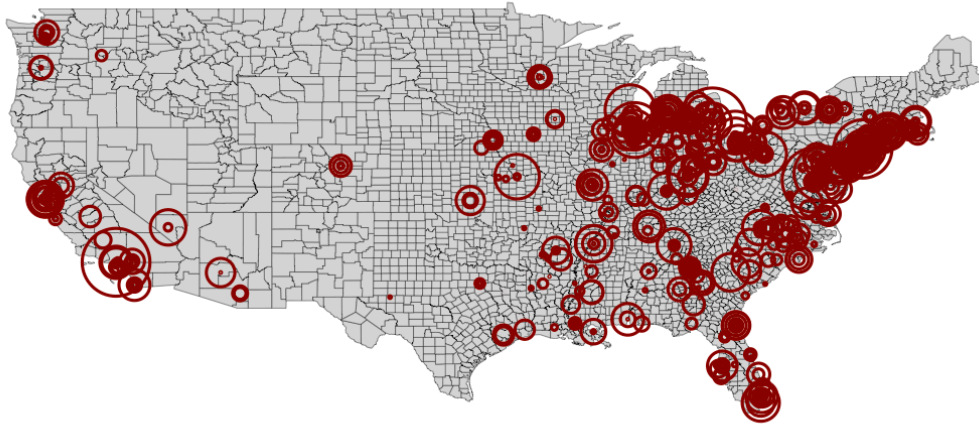


Figure 2.4.1: Map of the geographic distribution and severity of urban uprisings, 1964-1971. More severe riots are indicated by a larger radius. Bullseye-like circles indicate multiple riots of varying severity within the same city.

The riot treatment is a composite value that incorporates measures of distance, time and riot intensity. The distance measure is a binary measure that takes the value one if the distance is equal to or under 200 miles and zero if the distance is over 200 miles.¹¹ As compared with some decay function in which the “strength” of the treatment declined with distance, a binary measure throws away much of the available data. A binary measure is used, however, to precisely delineate treated from control units. As the model relies heavily on distance between counties and riot cities, only states in the continental

¹¹All cities and riots were geocoded at the city-level using <http://batchgeo.com>’s interface to Google Maps.

U.S. are included and thus all counties in Alaska and Hawaii are excluded. Figure 2.4.2 presents a plot of the estimated riot ‘treatment’ for every county by year. Each dot in the plot represent a single county in a single year. The height of the dot indicates the estimate riot ‘treatment’ for that county. Finally, the dots are translucent and jittered slightly so that it is easier to visualize the distribution of observations (i.e. the darkest swaths have the densest concentration).

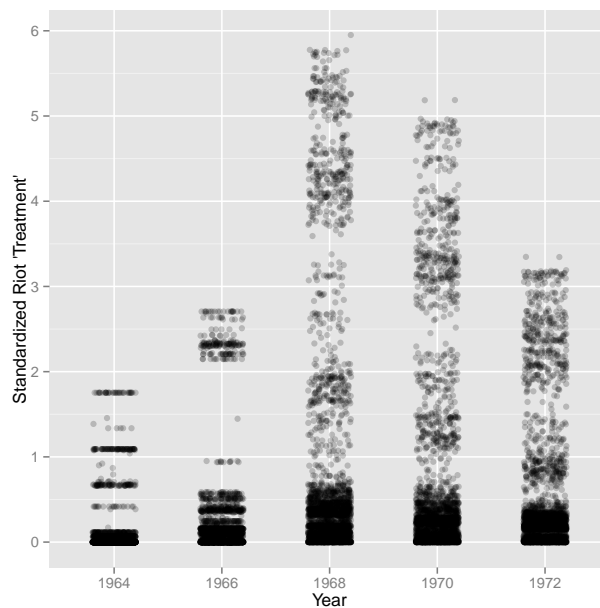


Figure 2.4.2: Scatter plot of the estimated riot ‘treatment,’ by year for all counties. Each dot represents one county in one year with the observations slightly jittered for better data visualization.

The time measure calculates the number of days between the date of the riot and the date of the relevant November election between 1964 and 1972. An inverse decay function is also applied to the time measure so that riots occurring closer to the election have a substantially greater “effect” than do riots occurring at earlier times. The last

component of the riot treatment is an intensity measure that accounts for the severity of the riot. This analysis uses the number of arrests that occur at the riot event. The number of arrests is scaled by taking the cube root.

Table 2.4.1: Summary Statistics

Variable	Min	Median	Mean	Max	SD
1960 County Population	208.00	18,409.00	57,441.60	6,038,771.00	204,268.10
1960 Birth Rate per 1,000	5.80	22.50	22.80	51.60	4.60
1960 County % Black	0.00	0.00	0.10	0.80	0.20
Riot Arrests	0.00	10.00	92.70	7,772.00	506.60
Riot Injuries	0.00	2.00	17.10	1,158.00	93.30
Riot Acts of Arson	0.00	1.00	21.20	3,000.00	143.90
Riot Deaths	0.00	0.00	0.30	43.00	2.30
Days of Rioting	1.00	2.00	2.40	13.00	1.90
1964 Riot 'Treatment'	0.00	0.00	6.70	141.30	22.80
1966 Riot 'Treatment'	0.00	0.20	17.90	218.10	44.40
1968 Riot 'Treatment'	0.00	22.40	71.20	479.70	118.50
1970 Riot 'Treatment'	0.00	18.60	57.40	418.10	95.60
1972 Riot 'Treatment'	0.00	14.80	39.20	269.80	63.10

Additional county-level data on resident population, racial composition, and birth rate are taken from the 1960 and 1970 censuses. For 1964, 1966, and 1968, I estimate linear-interpolations of the resident population and percent black between 1960 and 1970.¹² Summary statistics for the riot 'treatment' composite and the demographic data are presented in Table 2.4.1.

The census provides data on the number of white and black residents within each

¹²According to GeoLytics, Inc. (2009), more detailed data is not available at the county-level until the 1970 census. Future extensions will attempt to incorporate non-census data from "County and City Data Books" (Inter-University Consortium for Political and Social Research, 2001) as well as data from other state-level or SMSA-level sources.

county. From these data, an estimate of the percentage of the county that is black is calculated (no data is available in 1960 on resident population that is neither white nor black). The relationship between the racial composition of a county and the change in Democratic vote share appears to be non-linear and so a squared term is included as well.

2.5 MODELING THE EFFECT OF VIOLENT PROTESTS ON VOTING

Using ordinary least squares, I estimate the relationship between exposure to riots and voting behavior in as follows:

$$\begin{aligned} vote_i = & \alpha + \beta_1 treat_i + \beta_2 perblack_i + \beta_3 (perblack_i)^2 + \\ & + \beta_4 \log(pop_i) + \beta_5 births_i + \beta_6 lag_i + \beta_7 x_i + \varepsilon_i \end{aligned} \quad (2.1)$$

The term $vote_i$ is the percentage of the vote received in county i by the Democratic presidential candidate in a given election year. The term $treat_i$ represents the riot ‘treatment’ for county i in that election year and is defined in more detail in Equation 2.3. The $perblack_i$ term measures the percent of each county that is black and the $perblack_i^2$ term is the same measure squared. The $\log(pop_i)$ term is the log of the county population and the $births_i$ term measures births per 1,000 county residents in 1960. Finally, the lag_i term is the mean Democratic vote share over the last twelve years in either presidential elections (i.e. four prior elections) or congressional elections (i.e. seven prior elections). The x_i term represents other determinants of county-level voting behavior, particularly region-level or state-level fixed effects and ε_i is the error term.

I also estimate a panel model of the relationship between the riot ‘treatment’ and

voting in presidential and congressional elections. This panel model is similar to that in Equation 2.1 but each observation is a unique county-year pair and the model includes county and year fixed effects. The panel model is specified in Equation 2.2 and, where appropriate, the terms are indexed for the year j in which they are observed. Thus, $vote_{ij}$ represents the Democratic vote share in county i in year j .

$$\begin{aligned}
 vote_{ij} = & a + \beta_1 treat_{ij} + \beta_2 perblack_{ij} + \beta_3 (perblack_{ij})^2 + \\
 & + \beta_4 \log(pop_{ij}) + \beta_5 births_i + \beta_6 lag_i + \beta_7 x_i + \varepsilon_i
 \end{aligned} \tag{2.2}$$

I estimate the riot treatment as:

$$treat_{ij} = \sum_{k=1}^n distance_{ik} \times time_{jk} \times intensity_k \tag{2.3}$$

To understand Formula 2.3, it is useful to begin with a single county, riot and year. First, a distance is calculated between the county-riot pair. Second, the time in days is calculated between the riot and the election in the given year. Finally, the riot intensity is estimated from the number of arrests at the riot. The three measures are each scaled and then multiplied to produce a specific composite county-riot-year ‘treatment.’ All other things being equal, a county will be exposed to a stronger ‘dose’ whenever a riot is nearer geographically, occurs closer to election day or has a larger number of people arrested. Within a given year, this process is then repeated for all county-riot dyads. Ultimately, for each county, the final riot ‘treatment’ is the sum of every individual riot ‘effect’ on that county that has occurred before the relevant election day.

More precisely, in the model, each county i receives a unique ‘treatment’ i in year j . The $treatment_{ij}$ is the cumulative ‘effect’ or sum of all k riots occurring prior to the election in year j . The $distance_{ik}$ is a binary measure that equals one when the each riot k and each county i are at least within 200 miles of each other and zero otherwise. The $time_{jk}$ is an inverse scaled number of days between riot j and the election day in year k . The $intensity_j$ is a scaled number of arrests in riot j . $time_{jk}$ is scaled by a non-linear decay function.

2.6 ESTIMATING THE EFFECT OF A RIOT ‘TREATMENT’ ON DEMOCRATIC VOTE SHARE

Table 2.6.1 presents the estimated association between the year-specific riot ‘treatment’ and the county-level Democratic presidential vote share in the 1964, 1968 and 1972 elections. In 1964, a one unit increase in the riot ‘treatment’ is associated with about a -3.0 percentage point change in the county-level Democratic vote share (95% CI: -4.7, -1.2). Though statistically significant at conventional levels, the 95% confidence interval is wide, perhaps owing to the small number of riots occurring prior to the 1964 presidential election.¹³ In 1968, a one unit increase in the riot ‘treatment’ is more precisely estimated at about a -0.5 percentage point change in the Democratic vote share (95% CI: -0.8, -0.3). Likewise, in 1972, a one unit increase in the riot ‘treatment’ is associated with an approximately -0.6 percentage point change in the Democratic vote

¹³In 1964, most counties are estimated to have received almost no riot ‘treatment.’ Therefore, the 1964 presidential election may represent the best comparison of ‘treated’ and control counties (See Table 2.4.1). By 1968, according to the model, all counties experienced some degree of exposure. Thus, the large estimated effect of riots on Democratic vote share in 1964 may represent a more accurate assessment of the effect of riots on voting behavior and the later elections may be biased downward by the absence of a true untreated control group. I thank Michael Tesler for this insight.

Table 2.6.1: Riot ‘Treatment’ on Democratic Presidential Vote Share, 1964 to 1972

	1964	1968	1972
Riot ‘Treatment’	−2.97* (0.88)	−0.53* (0.12)	−0.61* (0.23)
Percent Black	−32.73* (4.20)	−61.13* (3.77)	−39.28* (3.73)
(Percent Black) ²	35.29* (7.03)	141.75* (6.28)	91.21* (6.22)
Log(Population)	0.26 (0.20)	2.02* (0.18)	2.41* (0.18)
Birth Rate	0.07 (0.06)	0.23* (0.05)	0.17* (0.05)
Mean Dem. Vote Share (‘48-’60)	0.60* (0.01)	0.32* (0.01)	0.22* (0.01)
Percent Pop. Growth	−0.01 (0.01)	−0.05* (0.01)	−0.03* (0.01)
Median Age	0.19* (0.06)	0.24* (0.05)	0.05 (0.05)
Median Income (000s)	2.21* (0.28)	1.48* (0.26)	0.82* (0.26)
Median Years Schooling	−1.59* (0.22)	−0.58* (0.20)	−2.01* (0.19)
Percent Unemployed	0.43* (0.07)	0.78* (0.07)	0.51* (0.06)
Percent Foreign Born	0.16* (0.06)	0.57* (0.06)	0.41* (0.06)
Per Capita Gov. Revenue	0.01* (0.00)	0.01* (0.00)	0.02* (0.00)
<i>N</i>	2914	2914	2912
<i>R</i> ²	0.68	0.50	0.43
adj. <i>R</i> ²	0.68	0.49	0.43
Resid. sd	8.93	7.98	7.90

Standard errors in parentheses

* indicates significance at $p < 0.05$

Riot ‘Treatment’ calculated separately for each election

Regional controls not presented

share (95% CI: -1.1, -0.1).

The summary statistics presented in Table 2.4.1 show that the estimated riot ‘treatment’ for 1968 ranges from a minimum of 0 in Apache, AZ (far from any significant riot activity) to a maximum of about 6 in Potter, PA (close to one of the largest riots in Newark, NJ). Thus, the results in Table 2.6.1, suggest that exposure to riots may have resulted in a shift in Democratic vote share from about 0 percentage points in the least exposed counties to about -3.2 percentage points in the most highly exposed counties. In sum, these results suggest that exposure to riots significantly influenced county-level presidential voting patterns between 1964 and 1972.

Table 2.6.2 presents the estimated association between the year-specific riot ‘treatment’ and the county-level Democratic congressional vote share in the five elections between 1964 and 1972. Unlike the presidential election results, in 1964 and 1966, the riot ‘treatment’ exhibits a positive and insignificant relationship with Democratic vote share. Beginning with the 1968 election, however, the results in Table 2.6.2 suggest that a one standard deviation increase in a county’s exposure to the riot ‘treatment’ was associated with a -1.1 percentage point change Democratic vote share in congressional elections (95% CI: -1.5, -0.7). Similarly, in 1970 a one unit increase in a county’s exposure to the riot ‘treatment’ was associated with a -1.6 percentage point change Democratic vote share in congressional elections (95% CI: -2.4, -0.9) and, in 1972, a -1.7 percentage point change Democratic vote share (95% CI: -2.8, -0.7).¹⁴

¹⁴One difference between the presidential and the congressional model is that, in the latter case, the data appeared to be significantly noisier than in former. To address a non-trivial number of districts with extreme values (i.e. almost all the votes for one party), counties with a lagged mean Democratic vote share (again, the past seven elections) less than 5 or greater than 95 are dropped from the analysis. The results are robust to this change.

Table 2.6.2: Association between Riot ‘Treatment’ and Democratic Congressional Vote Share, 1964 to 1972

	1964	1966	1968	1970	1972
Riot ‘Treatment’	0.87 (0.89)	-0.78 (0.51)	-1.11* (0.21)	-1.65* (0.36)	-1.70* (0.54)
Percent Black	7.57 (6.61)	-8.25 (9.54)	-22.25* (9.89)	16.93 (12.43)	-43.79* (12.40)
(Percent Black) ²	-4.69 (13.97)	3.03 (19.88)	19.20 (19.74)	-61.00* (25.85)	64.45* (25.82)
Log(Population)	0.69* (0.22)	0.95* (0.33)	0.44 (0.33)	1.24* (0.43)	2.40* (0.43)
Birth Rate	-0.01 (0.06)	-0.01 (0.09)	-0.21* (0.09)	-0.22 (0.12)	-0.04 (0.12)
Mean Cong. Dem. Vote Share (‘50-’62)	0.59* (0.01)	0.97* (0.02)	0.75* (0.02)	0.63* (0.03)	0.86* (0.03)
Percent Pop. Growth	-0.02* (0.01)	0.03* (0.01)	-0.05* (0.01)	-0.02 (0.02)	-0.03 (0.02)
Median Age	0.04 (0.07)	0.18 (0.10)	-0.21* (0.10)	-0.20 (0.13)	0.37* (0.13)
Median Income (000s)	0.79* (0.31)	-0.25 (0.46)	1.66* (0.46)	0.65 (0.61)	0.09 (0.61)
Median Years Schooling	-0.90* (0.24)	-0.64 (0.36)	-0.27 (0.35)	-1.83* (0.46)	-1.19* (0.46)
Percent Unemployed	0.60* (0.08)	0.79* (0.12)	0.83* (0.11)	0.71* (0.15)	0.67* (0.15)
Percent Foreign Born	0.12 (0.06)	0.38* (0.10)	0.13 (0.10)	0.19 (0.13)	0.10 (0.13)
Per Capita Gov. Revenue	0.00 (0.00)	0.04* (0.01)	0.03* (0.01)	0.04* (0.01)	0.01 (0.01)
N	2248	2424	2238	2401	2421
R ²	0.57	0.60	0.47	0.37	0.44
adj. R ²	0.57	0.60	0.47	0.37	0.44
Resid. sd	8.85	13.65	12.77	17.44	17.47

Standard errors in parentheses

* indicates significance at $p < 0.05$

Counties with mean Dem. vote share < 5 or > 95 excluded from analysis

Riot ‘Treatment’ calculated separately for each election

Regional controls not presented

Between 1968 and 1972, the results in Table 2.6.2, suggest that exposure to riots may have resulted in a shift in Democratic vote share from a low of 0 percentage points in the least exposed counties of the 1968 congressional election (again, Apache, AZ) to a peak of about -5.7 percentage points in the most highly exposed counties of the 1972 congressional election (such as Columbia, PA).

Table 2.6.3: Panel Models of Association between Riot ‘Treatment’ and Democratic Presidential and Congressional Vote Share, 1964-1972

	Presidential Elections	Congressional Elections
Riot ‘Treatment’	-0.46*	-2.04*
	(0.14)	(0.20)
Percent Black	359.06*	52.90
	(27.63)	(41.45)
(Percent Black) ²	-883.64*	-91.83
	(33.91)	(51.10)
log(Population)	2.29	-6.98*
	(1.97)	(2.94)
Year: 1966		-3.06*
		(0.35)
Year: 1968	-20.40*	-2.24*
	(0.23)	(0.40)
Year: 1970		0.70
		(0.39)
Year: 1972	-24.19*	-1.96*
	(0.22)	(0.37)
N	9271	14879
R ²	0.86	0.76
adj. R ²	0.80	0.70
Resid. sd	7.48	13.29

Standard errors in parentheses

* indicates significance at $p < 0.05$

Table 2.6.3 presents the results of the panel model estimating the association between the riot ‘treatment’ and Democratic vote share. As the panel model uses fixed effects for each county, any unobserved non-varying county-level factors should be addressed. Similarly, the panel model uses year fixed effects to account for unobserved time trends in the data. As with the prior results, the riot ‘treatment’ is negatively and significantly associated with Democratic vote share in presidential and congressional elections between 1964 and 1972. For presidential elections, a one standard deviation increase in the riot ‘treatment’ is associated with a -0.7 percentage point change Democratic vote share (95% CI: -1.0, -0.4). For the congressional elections, I find that exposure to riot activity is associated with -2.2 percentage point change Democratic vote share (95% CI: -2.6, -1.7)

2.6.1 COUNTERFACTUAL OF MARTIN LUTHER KING, JR. NOT ASSASSINATED

As riot occurrence was highly idiosyncratic (Spilerman, 1976; Olzak and Shanahan, 1996), many counterfactual scenarios could be envisioned in which fewer events of civil unrest occur. Figure 2.6.1 presents the expected allocation of electoral vote in the 1968 presidential election under one such plausible counterfactual. In this scenario, I estimate a revised riot ‘treatment’ as if Martin Luther King, Jr. had not been assassinated on April 4th, 1968 and 136 riots had not occurred in the immediate wake of his death. (Figure 2.2.6 presents a bar chart of urban riots by month and shows the unusual distribution in April.)

To estimate the counterfactual election, I remove the 136 riots from April 1968 from the data and calculate first differences between the observed riot ‘treatment’ and the

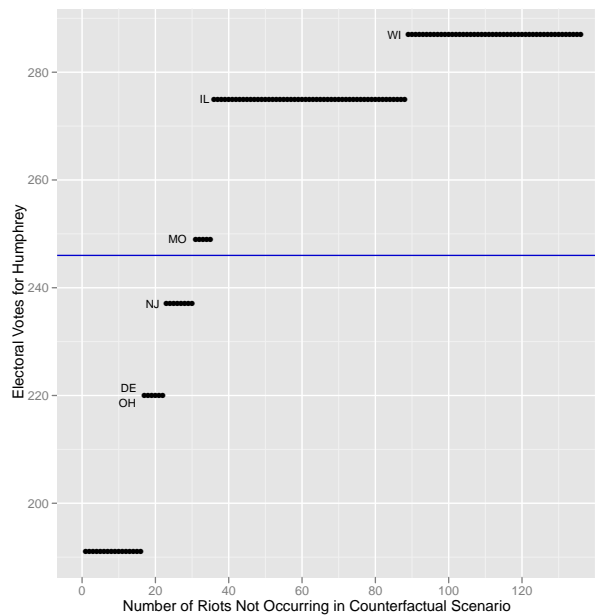


Figure 2.6.1: Plot of the estimated allocation of electoral votes for Humphrey under the counterfactual scenario of Martin Luther King, Jr. not being assassinated and 136 riots not occurring in April, 1968. The blue horizontal line indicates the number of electoral votes needed to win the presidency (assuming third party candidate George Wallace stays in the race receives the same number of electoral votes as in the actual 1968 election). At 270 electoral votes, Humphrey wins irrespective of what might have happened with Wallace.

counterfactual riot ‘treatment’ for each county in the continental United States. From this first difference, I estimate the change in the number Democratic votes in each county. For these estimates, increases to the Democratic vote total were assumed to come only at the expense of the Republican vote total and not from those of George Wallace’s third party candidacy.¹⁵ Aggregating the counterfactual vote totals for each county then allows for an estimate of the state-level vote totals and, ultimately, the winner of the state’s electoral votes. This process is then repeated for each additional violent protest until a counterfactual scenario in which all 136 riots are assumed to have not occurred. By re-estimating each candidate’s allocation of electoral votes under the counterfactual of fewer riots, it is possible to see if the Republican candidate, Richard Nixon, or the Democratic candidate, Hubert Humphrey, would have carried the election.

Under the counterfactual scenario in which 136 riots in April, 1968 had not occurred, I estimate that Humphrey would have won an additional 1.4 million votes nationally and a majority of the votes in six additional states: Delaware, Illinois, Missouri, New Jersey, Ohio and Wisconsin. These swing states would collectively have provided Humphrey with an additional 95 electoral votes and allowed him to win the 1968 election comfortably with a total of 286 electoral votes.¹⁶

Figure 2.6.2 presents a map of the allocation of electoral votes in the 1968 presidential election under the counterfactual scenario of Martin Luther King, Jr. not being assassinated and 136 riots not occurring in the wake of his death. As can be seen in

¹⁵Future extensions of this paper will attempt to model the reallocation of votes in a three-way race using a multinomial model, Monte Carlo simulations, as well as counterfactual elections at the congressional level.

¹⁶As Alaska, Hawaii and the District of Columbia are excluded from the statistical model, they are assumed to remain unchanged with Nixon carrying Alaska’s three electoral votes and Humphrey carrying Hawaii and D.C.’s seven.

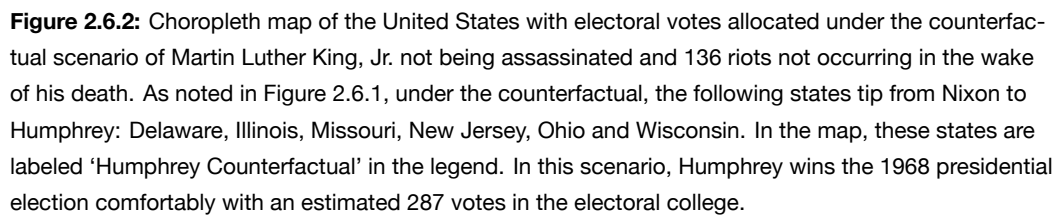


Figure 2.6.2, none of the states Humphrey is estimated to pick up (i.e. Delaware, Illinois, Missouri, New Jersey, Ohio and Wisconsin) are Southern.¹⁷ While Nixon is widely credited with having won the 1968 election with a “Southern Strategy,” in actuality, he wins very little of the South and, in these counterfactuals, the swing states are mid-Atlantic and Midwestern. A more accurate interpretation of the Southern Strategy in 1968 is that it helped Nixon peel off white moderate voters in middle America but did little for him in the South. In 1968, the third party candidacy of George Wallace carries the deep south but, had George Wallace not run and all of the deep south states gone to Nixon, Humphrey would still have won under the counterfactual. In 1964, a coalition of white liberals, white moderates and blacks helped the Democratic party win the presidency decisively. In 1968, by splitting that coalition and pulling white moderates outside of the south into the Republican fold, Nixon prevailed.

2.7 EXTENSIONS

While these results suggest a plausibly causal relationship, the association between riots and voting found here does not provide conclusive evidence of a causal relationship. It may be possible to use an instrumental variable approach developed by Collins et al. (2004) to evaluate the effect of riots on partisanship. Martin Luther King, Jr. was assassinated in April, 1968 and, partly in response, a record 136 riots occurred over the course of the month. Using city-level weather data from April 1968 as an instrument for whether a riot occurred, I intend to investigate if urban riots caused a partisan shift in voting behavior between the 1964 and 1968 presidential elections. Results from this

¹⁷Missouri is classified as part of the Midwest by the US Census Bureau.

analysis could be the first causal assessment of the effect of riots on American political outcomes.

An additional question that remains unresolved by this analysis is why “law and order” remained a politically salient issue for decades despite a collapse in crime’s perceived importance. More narrowly, why did the early federal measures evolve into a durable set of anti-crime policies that dramatically increased imprisonment? A clearer mechanism by which crime has remained salient in political campaigns and evolved into policy innovations like the war on drugs is essential though beyond the scope of this paper.

2.8 VIOLENT VERSUS NON-VIOLENT PROTEST

The evidence presented thus far suggests that violent protests by blacks may have played an important role in shifting support away from Democrats and towards a desire for more repressive law enforcement. The lasting consequences of this shift have contributed to the rise of one of the most repressive law enforcement regimes in the world (Gottschalk, 2006). Put simply, in the post-civil rights era, black militancy may have been justified but was not strategic.

By contrast, the moderate strategies of the earlier civil rights period appear to have been highly effective. Again, put simply, in response to brutal acts of racism, non-violent civil disobedience may have been unjustified but was strategic.¹⁸ As shown in

Figure 2.2.4, public concern with civil rights jumped dramatically between March and

¹⁸On the justifiability of nonviolence, for example, Malcolm X has argued “Concerning nonviolence: It is criminal to teach a man not to defend himself, when he is the constant victim of brutal attacks. It is legal and lawful to own a shotgun or a rifle. We believe in obeying the law.” Along similar lines, Malcolm X also states, “I don’t call it violence when it’s self-defense, I call it intelligence” (Malcolm X, 1965).

September 1963. In the middle of 1963, a handful of key events occurred that may have shifted public opinion toward prioritizing civil rights. In May, non-violent civil rights protestors were attacked by dogs and fire hoses while protesting discrimination in Birmingham, Alabama. Images of the brutal repression by Commissioner of Public Safety Eugene “Bull” Connor were broadcast around the world. In June, Mississippi’s NAACP field secretary, Medgar Evers, was murdered outside his home. In August, approximately 200,000 people attended the peaceful March on Washington. Finally, in September, four young black girls attending Sunday school were killed when a bomb exploded at the Sixteenth Street Baptist Church, a location previously used by civil rights organizers. Much as political violence associated with black rioters may have fueled demand for social control in the latter-half of the 1960s, it appears plausible that violence associated with white supremacists and nonviolence associated with blacks in the first half of the 1960s may have moved moderate whites towards elevating civil rights to the position of most important problem in America.

In his 1964 presidential nomination speech, Barry Goldwater received a rousing response when he bellowed, “Extremism in the defense of liberty is no vice. And let me remind you also that moderation in the pursuit of justice is no virtue” (Flamm, 2005). The evidence presented in the paper, however, suggests that, in the context of U.S. democratic politics, violent extremism may do more harm than good to its own cause. This finding echoes that of Stephan and Chenoweth (2008) who find that non-violent movements are successful at achieving their goals about 50 percent of the time and violent movements about 25 percent of the time.¹⁹ Similarly, in post-war America,

¹⁹They also draw a distinction between strategic nonviolence and principled nonviolence which they describe as the product of ethical or religious beliefs.

moderate non-violent civil disobedience may have been highly virtuous in terms of its ability to elevate the agenda of a persecuted minority to a just cause supported by a winning coalition of Americans.

2.9 APPENDIX

2.9.1 VISUALIZING THE DISTANCE FUNCTION

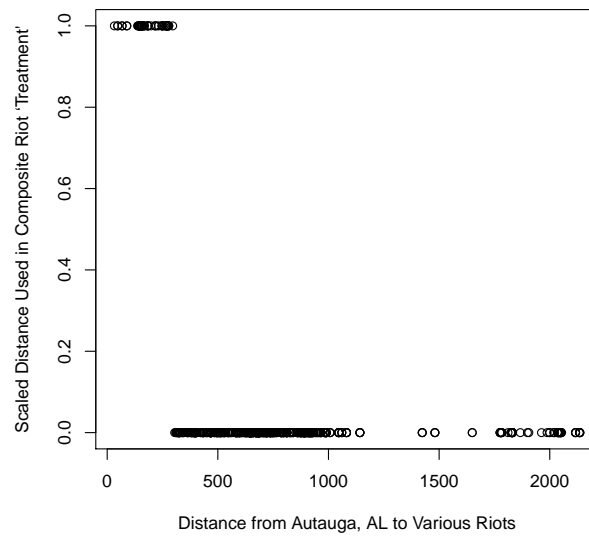


Figure 2.9.1: Plot of the distance decay function for one county

2.9.2 BLACK PARTY IDENTIFICATION, 1936-2004

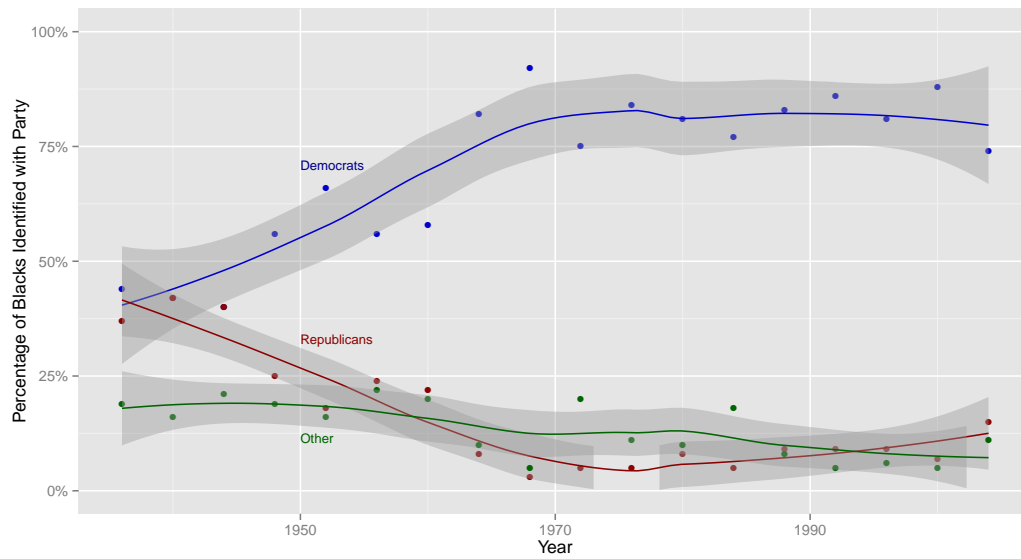


Figure 2.9.2: Line plot of black party identification, 1936 to 2004. Sources: (Bositis, 2008). Lines drawn with loess smoothing function.

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3

Triggering Punitiveness

CAN MEDIA CUES ABOUT RACE AND POLITICS INFLUENCE DOMINANT GROUP IDENTITY,
BEHAVIOR AND ATTITUDES?

3.1 INTRODUCTION

In the 1960s and early 1970s, more than 750 incidents of black-led civil unrest exploded across American cities. At the same time, in elections across the country, moderate white voters abandoned the Democratic party in large numbers and increasingly voted for Republican candidates promising “law and order.” Whether the 1960s black uprisings influenced rising white conservatism remains a subject of scholarly debate (Weaver, 2007). Did moderate white voters shift to the right in opposition to landmark civil rights legislation or in response to violent protests and perceptions of growing social disorder? Was white flight from the Democratic party driven more by elite or mass behavior? Why was the issue of “law and order” captured by Republicans rather than Democrats who, under President Johnson, initiated the first national “war on crime”?

Looking at Congressional and Presidential elections between 1964 and 1972, Wasow (2012) offers evidence that, at the county-level, black-led violent protests caused a decline in support for the Democratic party. Building on the substantial literature around “racial threat,” Wasow proposes that fear of violence by an outgroup (in this case, blacks), or “perceived existential threat,” changed voting behavior by an ingroup (whites). In short, echoing work in comparative politics on riots and ethnic conflict in India (Wilkinson, 2004; Varshney, 2003), Wasow argues that violent nationalist protests by a subordinate group triggered a form of reactionary nationalism in the dominant group.

How did urban uprisings influence voting behavior? Wasow (2012) proposes two main effects. First, violent protests by blacks increased the political salience of racial boundaries and identities. Second, civil unrest by blacks increased concerns about social

control and the ability of the state to protect members of the white ingroup. He writes, “In essence, the urban riots provoked many whites to become more white-identified and triggered a Hobbesian mindset in many white voters that prioritized ingroup order and safety above other priorities.” Though evidence suggests both dynamics are at play, aggregated information like county-level voting data, public opinion polls and gun sales statistics offer only an indirect macro case. At the micro-level, Wasow presents little direct evidence that any individual in the 1969s perceived racial or existential threat.

To test if perceived existential threat operates at a micro-level, I conduct a lab experiment to assess the degree to which exposure to violence by a subordinate group might increase the salience of ethnic or racial identities in the dominant group as well as change attitudes about issues of security. In the context of the 1960s, such a result might help explain why a significant subset of white voters switched from the Democratic party, which became identified with supporting black interests, to the Republican party, which embraced policies and rhetoric associated with white interests.

3.2 WHAT DRIVES RACIAL THREAT?

In 1949, V. O. Key observed that as the percentage of black residents in Southern counties increased, whites tended to turn out to vote at higher rates. What Key described as a kind of “Negrophobia,” is now commonly referred to as “group threat” or “racial threat” and is generally understood to flow from a feeling of animus towards or competition with an outgroup that threatens the material interests, safety or social status of one’s ingroup (Key, 1949; Blalock, 1967; Cullen and Wilcox, 2010). Despite a substantial literature, the evidence in favor of the racial threat hypothesis is ambiguous. For example, Enos (2011)

notes that though electoral competition has often been presumed to be a key driver of racial threat, at the time of Key's analysis few blacks could vote in the Jim Crow South.

Many observational studies have confirmed Key's initial finding of a positive relationship between outgroup population size and ingroup voter mobilization (Blumer, 1958; Quillian, 1995; Giles and Hertz, 1994; Hill and Leighley, 1999; Enos, 2010). Other studies, however, have not supported the racial threat hypothesis (Voss, 1996; Leighley and Vedlitz, 1999; Bledsoe et al., 1995). Finally, some studies have found both positive and negative evidence. Enos (2011) conducts a field experiment in Los Angeles and finds evidence of increased African American turnout when exposed to cues of racial threat from Latinos but not the converse as Latinos are unresponsive to signals of racial threat by African Americans. Barber and Imai (2013) geocode over 25 million voter registration records in California and Florida and find that, overall, a 10 percentage point increase in the outgroup in a voter's neighborhood leads to a one percentage point decrease in the probability of turnout. Barber and Imai's effects, though, differ by racial group with black turnout negatively affected by the size of the outgroup and white turnout generally increasing.

Though theories of racial threat have been refined over time (Bobo and Hutchings, 1996), the persistence of conflicting results suggest that racial threat may be more of a contextual than a universal phenomenon and that scholars should shift from debating the existence of the dynamic to identifying the situations in which group threat operates or is stimulated. Wasow (2012) argues that violent protests by an outgroup may be one such situation that provokes feelings of racial threat and this study attempts to test the validity of that hypothesis by investigating the role of violent and politicized imagery in driving

feelings of ingroup identification and a desire for social control.

3.2.1 INGROUP IDENTIFICATION

Haidt (2012) argues that the human capacity for “groupism,” be it religious, ethnic, or along some other dividing line, is an evolved solution to collective action problems in which it is advantageous to have individuals sacrifice for the greater good of the whole. In his words, humans are “90 percent chimp and 10 percent bee” and our capacity for hive-mind like behavior and cooperation has been enormously adaptive in evolutionary terms. Work on Social Identity Theory has also shown that individuals will sacrifice for the benefit of groups Tajfel et al. (1971). Building on Social Identity Theory, Dawson (1994) demonstrated that an individual’s sense of “linked fate” to a larger group can also lead to behavior that overcomes collective action problems. Whether viewed within Haidt’s framework or Social Identity Theory, racial threat can be seen as one of many processes that activate the bee or group-identified part of the human psyche.

What role does awareness of outgroup violence play in racial threat? Perceptions of violence and crime that cleave along group lines appear to be one powerful way to “activate the bee.” Eitle, D’Alessio and Stolzenberg (2006) disaggregate racial threat into three conceptually distinct forms and find strong support for a sense of threat among whites from perceptions of black-on-white crime (but not from black-on-black crime). Economic threats and political threats, by contrast, appear to exert little power in their South Carolina sample. Gilliam and Iyengar (2000) experimentally investigate the intersection of race, violence and media and find that local news reporting of violent crime “racializes” political discourse” by intertwining public opinion on policy with

attitudes about race. Rahn and Transue (1995) explore the political significance of fear of crime and find that, for socially isolated individuals, fear of crime is largely driven by media and erodes faith in government responsiveness. Though each of these studies illuminates an aspect of how race and violence intersect to drive feelings of racial threat, the underlying psychological mechanisms at play remain obscure. More narrowly, Wasow (2012)'s hypothesis that cues about outgroup violence can influence ingroup identification is untested.

Does exposure to violent protests provoke a different threat response than that of violent crime? In addition, do race, violence and politics intersect in ways that trigger different feelings of threat? Sidanius and Pratto (2001) extend Social Identity Theory to account for the recurring patterns hierarchy among social groups. They argue that mass violence or politicized violence by a subordinate group may be a particularly potent way to activate feelings of group threat:

“...violence against dominants at the hands of subordinates will be considered more serious because such violence reaches beyond mere criminality and constitutes acts of sociopolitical insubordination. Such violent insubordination becomes a potential threat to the stability and integrity of the group-based system of social hierarchy itself” (p. 215).

Beyond Sidanius and Pratto (2001), though, little has been done to test how political content and violence interact when assessing feelings of racial threat.

Where most of the scholarship on racial threat explains the phenomenon through some form of antipathy to an outgroup, Knowles and Peng (2005)'s results suggest the possibility of affinity to an ingroup as an independent (though, likely, often interdependent) force to consider. In contrast to these models of group identity forged

primarily in competition and conflict, Knowles and Peng find that awareness of one's own membership in an ingroup may depend substantially on the presence of an outgroup. For example, they find that growing up in more racially diverse areas is associated with scoring higher on the White Identity Centrality Implicit Association Test (explained in more detail later). While Knowles and Peng's results might also be driven by intergroup competition and conflict, nothing in their model presumes such a dynamic. Their model proposes that a person's sense of white identification "derives (in part) from individuals' social exposure to non-Whites and subsequently shapes individuals' cognitions and emotions by binding evaluations of the self to those of the ungroup" (p. 223). Echoing the work of Barth et al. (1970), Knowles and Peng's work suggests that the meaning and salience of identity is highly contextual and significantly informed by interactions with outgroups.¹ How much of racial threat is driven by ingroup affinity versus outgroup antipathy remains an open question.

3.2.2 PUNITIVENESS

Following Key (1949), much of the political science literature on racial threat focuses on voting behavior. In sociology, criminology and other fields, however, research on racial threat often investigates its effects on attitudes, particularly on issues like social control. Opinions about the appropriate level of punitiveness in the criminal justice system vary significantly by race (Bobo and Johnson, 2004). Using framing experiments, Bobo and Johnson (2004) find anti-black affect is a significant driver of the "taste for punishment" among whites. Racial threat, as measured by minority population size, also appears to

¹For more on the contextual nature of white identity, see McDermott and Samson (2005).

play an important role. A number of studies find that, even after accounting for a variety of confounding factors including homicide rates, the size of the minority population in a state is a significant predictor of its punitiveness as measured through indicators like the incarceration rate and the presence of a death penalty (Jacobs and Carmichael, 2002; Jacobs, Carmichael and Kent, 2005; Jacobs, Malone and Iles, 2012). The role of violent protests in triggering punitiveness is unclear, though.

3.2.3 A “CIRCLE THE WAGONS” EFFECT?

In contrast to more general theories of group threat, I extend Wasow (2012) to propose a narrower, contextual model in which perceptions of violence by an outgroup can provoke a “circle the wagon” mentality among members of the ingroup that heighten ingroup identity, increase outgroup antipathy and motivate greater concern for safety.² The “circle the wagon” phenomenon appears to operate a bit like a “rally ’round the flag” effect (Mueller, 1970) in which external conflict drives increased nationalist fervor and patriotism. Perceptions of an internal threat by a domestic outgroup differ from the “rally ’round the flag” dynamic, however, in a number of critical ways. First, these perceived conflicts are sub-national and typically work across cleavages of race and ethnicity rather than country. Second, rather than a surge of national pride, these perceived conflicts generate a heightened sense of allegiance to the ethnic or racial ingroup. Third, where the “rally ’round the flag” dynamic leads to greater support for a President or the state, these

²The idiom “circle the wagons” comes from the history of the early American West and is defined by Reference.com as a maneuver “to form the wagons of a covered-wagon train into a circle for defensive purposes, as against Indian attack” (Dictionary.com, 2013). Though the term can refer to more general forms of defensiveness and inward looking, its etymological roots in interracial conflict make it especially apt as a way to describe a type of group threat.

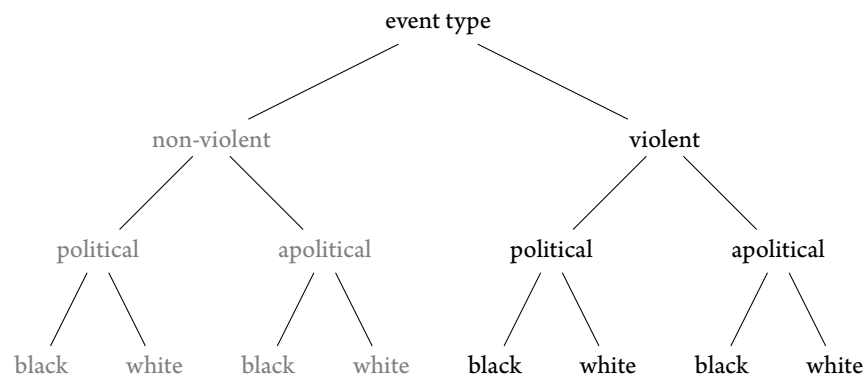


Figure 3.2.1: Typology of events by the tactics utilized, political orientation of event and race of participants. The first phase of this study focuses on variation in responses to violent acts when committed by blacks as compared with whites. The second phase of the study may focus on a broader range of event types including non-violent protests and apolitical gatherings (see gray text).

cases of racial threat correspond with feelings that the state is failing to adequately protect members of the ingroup and, therefore, lead to greater support for more punitive justice policy. Fourth, building on the earlier points, in the wake of perceptions of violence by an outgroup and a sense that the state has failed to ensure safety, members of the ingroup often take it upon themselves to provide security through actions ranging from moving to a new neighborhood to buying a gun to engaging in vigilante “rough justice.” Fifth, when the ingroup in question is the dominant group in the society and the outgroup is a subordinate group, perceptions of outgroup violence will likely spark more consolidated ethnic and racial bloc voting as well as a shift by a significant swath of the dominant group toward a more nativist party.

3.3 RESEARCH DESIGN

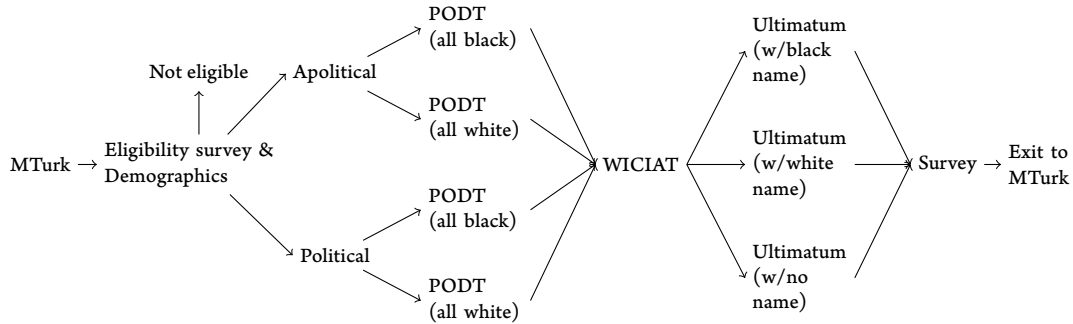


Figure 3.3.1: Subjects are recruited from Amazon Mechanical Turk (MTurk), provide basic demographic information, are randomized into the “apolitical” or “political” condition and then either receive the treatment or the control condition. Following treatment, subjects take the White Identity Centrality Implicit Association Test (WICIAT). After the WICIAT, subjects are randomized into one of three ultimatum game scenarios and, finally, complete some additional political and racial attitudes survey questions. Upon completion, subjects are returned to MTurk to be paid.

This study investigates how exposure to media with cues about ingroups, outgroups, violence and politics can shape identity, attitudes and behavior. Figure 3.2.1 presents three possible variations of interest: whether scenes depicted in media are primarily violent or non-violent, political or apolitical and primarily involve blacks or whites.³ This paper only analyzes variation in responses to violent imagery in which the political content and the race of actors is randomly assigned.⁴

Figure 3.3.1 presents a flowchart showing how subjects move through the experiment.

Overall, the design has three dimensions across which a subject could be randomly

³Another possible variable of interest to address in future research is the number of people involved in the incident. Conceivably, perceptions of violence by a few individuals might be much less threatening than violence by groups or many groups.

⁴The differential effects of non-violent vs. violent events will be the subject of future extensions to this research. In particular, it would be illuminating to test whether non-violent protests by a subordinate group produce a significantly different response in racial identification among members of the dominant group. Such a result might help explain Stephan and Chenoweth (2008)’s finding that non-violent movements are about twice as successful at achieving their goals than violent movements.

assigned. First, in the context of a simple videogame, subjects are randomly assigned to be shown images of black or white men. Second, within the same game, half of the images of men also contain political slogans and half of the images are apolitical. Finally, all subjects play a version of the ultimatum game in which the name of their co-player is randomly assigned to be either typically white, typically black or 'your partner.' Thus, the design is a 2 x 2 x 3 or 12 condition experiment. Each of the conditions is outlined in more detail below.

The outcomes of interest are the subject's score in the White Identity Centrality Implicit Association Test, their acceptance or rejection of an unfair offer in an ultimatum game and their responses to a series of survey questions. The survey questions assess ideology, punitiveness, fear of crime and racial attitudes. Almost all of the questions are taken with little or no change from past American National Election Studies (American National Election Studies, 2010). The survey questions are designed to tease out if the various stimuli about race, politics and violence cause meaningful differences in opinion.

3.4 DATA

Subjects for this study were drawn from Amazon Mechanical Turk's (MTurk) online labor market.⁵ Subjects were paid \$0.90 for completing a 20 minute survey (additionally, 38 percent of subjects received \$0.10 for accepting the offer in the ultimatum game). The study was described as a survey about "Videogames and public opinion." Approximately, 75 percent of the respondents self-reported being non-Hispanic white (and not of mixed

⁵In keeping with the results of Berinsky, Huber and Lenz (2012), I make no claims about the external validity of this national, unrepresentative convenience sample.

racial ancestry). After excluding non-white subjects from the respondent pool, 369 subjects remained. Table 3.4.1 presents summary statistics of selected demographic information for the white respondents. As the subjects were randomly assigned to the various treatment and control conditions, none of the demographic information presented in Table 3.4.1 is used in the statistical analyses that follow.⁶

Table 3.4.1: Summary Demographic Characteristics of White Respondents

	Variable	N	Mean	Min	Max	SD
1	Male	184	0.50	0.00	1.00	0.50
2	Democrat	156	0.42	0.00	1.00	0.49
3	Republican	64	0.17	0.00	1.00	0.38
4	Independent	121	0.33	0.00	1.00	0.47
5	Northeast	65	0.18	0.00	1.00	0.38
6	Midwest	93	0.25	0.00	1.00	0.43
7	South	131	0.36	0.00	1.00	0.48
8	West	80	0.22	0.00	1.00	0.41
9	High School / GED	339	0.92	0.00	1.00	0.27
10	BA	118	0.32	0.00	1.00	0.47
11	MA +	5	0.01	0.00	1.00	0.12
12	Self-reported Income	369	50,511.50	2,500.00	175,000.00	33,028.30

⁶In a future extension of this paper, I would like to considerably expand the number of subjects and attempt to subset the data by gender, region (i.e. South), income and other possible factors to see if other patterns emerge.

3.5 CAN EXPOSURE TO RACIALIZED AND POLITICIZED IMAGES CHANGE WHITE INGROUP IDENTIFICATION?

The videogame image experiments evaluate the effects on white identity of exposure to images of white and black armed and unarmed men. After completing a survey that collects standard demographic information, subjects played one of four versions of the Police Officer Dilemma Task (PODT) (Correll et al., 2002). In the PODT, still images of white or African American men holding guns or other objects appear in front of complex backgrounds and participants must decide in under one second between “shooting” armed targets or “not shooting” unarmed targets. In the original PODT, half of the the images are of white men and half of the images are of black men and the object of the study is to measure differences, by the race of the “target,” in how subjects respond to armed and unarmed men. See Figure 3.5.1 for example images of unarmed men.



Figure 3.5.1: Two sample images from the Police Officer’s Dilemma Task of an unarmed white man holding a cell phone and an unarmed black man holding a wallet in front of a complex background. Images like these, and those of armed men, are used in both the apolitical and political outgroup violence experiment.

For this study, the PODT is modified in two ways. First, subjects are divided into



Figure 3.5.2: Two sample images of an armed black man. The image on the left is used in the apolitical outgroup violence experiment and comes from the original PODT. The image on the right includes the slogan “Fight for Freedom” and comes from the political condition of the modified PODT.

“apolitical” and “political” conditions. In the apolitical condition, as in the original PODT, there is no explicit political content to the instructions or images. The unarmed men are described as “good guys” and armed men as “bad guys”. In the political condition, the instructions are modified so that instead of shooting “bad guys,” subjects are instructed to shoot members of a “militant political group.” Additionally, the images are modified to include protest signs or graffiti with political messages like “Ballot or Bullet” or “Fight for Freedom.”⁷ Figure 3.5.2 shows example images of an armed man in both the apolitical condition and political condition (note the sign in the background). Second, where the original PODT presents an even mix of white and black men, in the modified PODT subjects are randomly assigned to play the game seeing images only of white men or only of black men.⁸

⁷There are twenty slogans, the majority of which are taken from <http://www.teapartyslogans.com> and modified slightly to be as short as possible. All slogans were selected or edited to be plausibly applicable to both black and white political activists. The original PODT images were retouched to incorporate the slogans in ways that were appropriate to each photo. The list of slogans can be seen in Section 3.8.1.

⁸In a future extension, I would like to compare results from single race imagery to no race imagery and mixed race imagery. In addition, I would like the treatment to incorporate video imagery of actual protests. While a majority of the video footage might be ambiguous as to key details of the event (i.e.

Following exposure to one of the four videogame scenarios (again, see Figure 3.3.1 for the four PODT conditions) subjects complete the White Identity Centrality Implicit Association Test (WICIAT) to measure the degree to which they see themselves as members of a white ingroup (Knowles and Peng, 2005). In the context of this study, the WICIAT score is an indicator of the effects on white identity of exposure to cues about race, politics and violence. The WICIAT is an extension of the Implicit Association Test (IAT) developed by Greenwald, McGhee and Schwartz (1998). Knowles and Peng (2005) conceptualize white identification as, “the degree to which an individual has incorporated White ingroup membership into his or her self-concept” (p. 223).

In the test, subjects are asked to sort words like “I, me, myself” and “them, other, themselves” into the categories “Me” and “Not Me.” In addition, subjects are asked to sort images of black male and white male faces into the categories “White” and “Not White.” As with other implicit association tests, the WICIAT measures response latencies when subjects need to categorize stimuli into pairs of categories. Put another way, the WICIAT measures the degree to which a subject exhibits an automatic association between the concepts *White* and *self*.⁹

birds-eye views of large numbers of people massing) a few seconds of the video clip could cue the subject as to whether the event was political or apolitical, included violent or non-violent tactics and whether the primary actors were white or black. Another approach might be to use other less visceral but more easily manipulated media, such as newspaper clips, to represent a variety of events with text and still images.

⁹Knowles and Peng (2005) validate the WICIAT with four other tests. For example, they assess how exposure to an outgroup changes racial identification and find, “participants from largely non-White regions scored higher in implicit White identity centrality than did participants from predominantly White counties.” In a validation test in which subjects are asked to categorize racially ambiguous faces, “participants scoring high on the WICIAT tended to exclude racially ambiguous faces from the White ungroup ... more than did weakly identified participants.” Finally, in response to reading about the epidemic of black lynchings in American history, Knowles and Peng find “high-WICIAT participants experienced higher levels of negative self-evaluative emotion (i.e., guilt, shame, and embarrassment) than did low-WICIAT individuals.”

Table 3.5.1: Summary Statistics of White Identity Centrality

	Variable	Condition	N	Mean	Min	Max	SD
1	White Identity Centrality	All images	369	0.44	-1.66	1.69	0.46
2	White Identity Centrality	White images	182	0.53	-1.23	1.48	0.46
3	White Identity Centrality	Black images	187	0.36	-1.66	1.69	0.45
4	White Identity Centrality	Political images	188	0.43	-1.66	1.36	0.46
5	White Identity Centrality	Apolitical images	181	0.45	-1.23	1.69	0.46
6	White Identity Centrality	White & political images	94	0.54	-1.07	1.36	0.44
7	White Identity Centrality	White & apolitical images	88	0.51	-1.23	1.48	0.47
8	White Identity Centrality	Black & political images	94	0.31	-1.66	1.36	0.45
9	White Identity Centrality	Black & apolitical images	93	0.40	-0.91	1.69	0.44

Table 3.5.1 presents summary statistics of scores on the WICIAT for all subjects and for those subjects exposed to white images, black images, political images, apolitical images and the various combinations. As can be seen in Table 3.5.1, the mean WICIAT score varies substantially for those shown white images in the simple videogame as compared to those shown black images.¹⁰ The mean WICIAT score does not vary much, however, based on the political content of the images.

Figure 3.5.3 plots the estimated difference in mean WICIAT scores and 95 percent confidence intervals using Welch's Two Sample t-test. The first estimate is for the

¹⁰Unexpectedly, subjects shown the white images exhibit higher mean WICIAT scores than those exposed to the images of black men. In other words, ingroup imagery raised group identification and/or outgroup imagery lowered group identification. One possible explanation for this outcome is that, in disambiguating armed from unarmed men, the subject is forced to develop a more complicated and sympathetic view of the target group. Another possible explanation is that subjects will inevitably accidentally "shoot" an unarmed target and this may generate a feeling of empathy rather than antipathy. It may also be possible that exposure to racialized imagery creates some degree of identification with the images. Further testing is needed.

difference in means for subjects exposed to images of white men as compared with those exposed to images of black men. The second estimate is the difference in means for subjects exposed to images with political slogans as compared with those exposed to images with no political slogans.

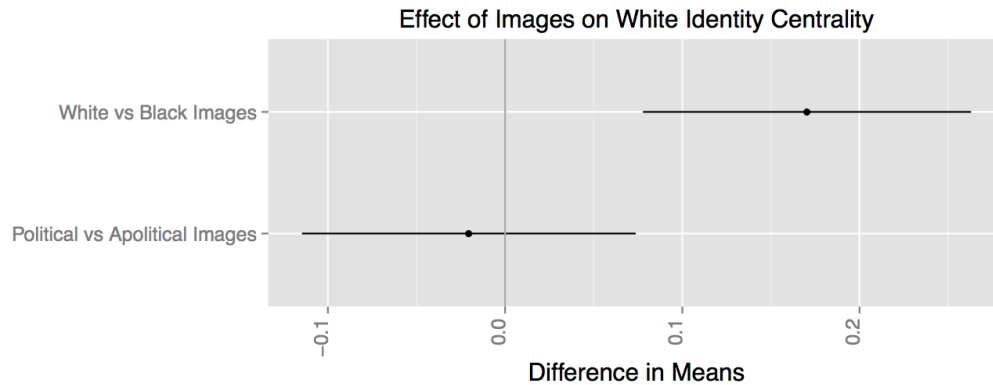


Figure 3.5.3: Difference in means for White Identity Centrality by white images versus black images and political images versus apolitical images.

What effect does exposure to images of armed and unarmed white men (as compared with similar images featuring black men) have on identification with a white ingroup? Figure 3.5.3 suggests that exposure to images of white men (as compared to black men) significantly increased subjects' White Identity Centrality measures by about 0.17 units. The scale for the WICIAT runs from -2.0 to 2.0 and the results in Figure 3.5.3 represent a shift of about 0.37 of a standard deviation for the subjects of the study. In substantive terms, an increase in the WICIAT score of 0.17 units would be enough to move someone from exhibiting "little or no" implicit association between themselves and a white ingroup to exhibiting "a slight" association. Or, it would be enough to shift many subjects from from "a slight" association between themselves and a white ingroup to "a moderate

association.”¹¹ Though neither shift is dramatic, both suggest that, at the margin, it is possible to meaningfully increase ingroup identification.

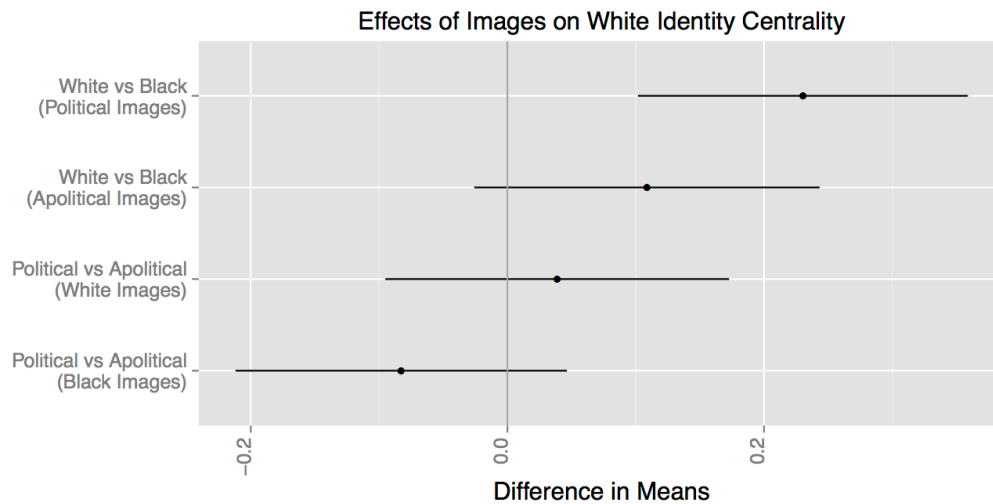


Figure 3.5.4: Difference in means for White Identity Centrality by interaction of racialized and politicized images.

Figure 3.5.4 disaggregates the results in Figure 3.5.3 to view the interaction effects of race and politics on White Identity Centrality. The results presented in Figure 3.5.4 suggest that the positive and significant effect of white imagery (relative to black imagery) on White Identity Centrality is driven largely by the combination of political content with race. Subjects exposed to white images with political slogans as compared to black images with political slogans, exhibited a positive and significant increase of approximately 0.23 in their WICIAT or 0.5 of a standard deviation within this sample.

The mean WICIAT scores in the three other conditions of the modified PODT are not

¹¹Conventional interpretation of the IAT suggests scores between 0 to ± 0.15 exhibit “little or no” automatic preference. Scores $> \pm 0.15$ but $\leq \pm 0.35$ exhibit “a slight” automatic preference. Scores $> \pm 0.35$ but $\leq \pm 0.65$ exhibit “a moderate” automatic preference and scores $\geq \pm 0.65$ are interpreted to exhibit “a strong” automatic preference.

statistically different from zero.

3.5.1 DOES WHITE IDENTITY CENTRALITY INDIRECTLY INFLUENCE OPINION?

Table 3.5.2: Logistic Regression of White Identity Centrality on Favoring Indefinite Detention for Suspected Terrorists without Due Process, by Exposure to Images

	White (All)	White & Political	White & Apolitical	Black (All)	Black & Political	Black & Apolitical
White Identity Centrality	0.02 (0.51)	0.48 (0.66)	-0.99 (0.81)	1.17* (0.52)	-0.15 (0.67)	2.70* (0.89)
<i>N</i>	182	94	88	187	94	93
AIC	134.17	89.23	41.05	149.35	75.75	70.49
BIC	159.81	109.58	60.87	175.20	96.10	90.76
log <i>L</i>	-59.09	-36.61	-12.53	-66.68	-29.87	-27.25

Standard errors in parentheses

* indicates significance at $p < 0.05$

Is there any substantive effect of an increase in a subject's WICIAT score? Two additional results suggests there is. Following exposure to the modified PODT and the ultimatum game, subjects were asked a series of survey questions. The first question was "Imagine that the U.S. government suspects a person in the United States of being a terrorist. Do you favor, oppose, or neither favor nor oppose the government being able to put this person in prison for months without ever bringing the person to court and charging him or her with a crime?"

Table 3.5.2 presents the results of six logistic regressions on the relationship between White Identity Centrality and favoring or opposing a more punitive criminal justice

policy for suspected terrorists.¹² The first three columns of Table 3.5.2, present the estimated relationship for subjects shown white images. The fourth through sixth columns present the results for those subjects shown similar images of black men. The regression coefficients for subjects shown white images are not statistically different from zero. The results in the “Black (All)” column present the relationship for those subjects exposed only to images of black men. In contrast to the results for those exposed to white images, showing subjects images of black men causes a significant, positive relationship between White Identity Centrality and favoring indefinite detention. For the median subject exposed to the images of black men, a one unit increase in White Identity Centrality is associated with an approximately 222 percent increase in the odds of favoring a harsh criminal justice approach to alleged terrorists.¹³¹⁴

Further, in disaggregating the effect of black images by their political content, it becomes clear that the positive and significant results for subjects shown black images are driven entirely by the effects of black and apolitical images. As Table 3.5.2 presents, the effect of showing subjects black and political images is statistically indistinguishable from zero. The effect of black and apolitical images is highly significant. It appears that the inclusion of political content in the treatment significantly attenuates the effect of exposure to black and apolitical images.

The results in Table 3.5.2 also offer some insight into possible mechanisms by which racial threat may operate. First, exposure to violence by an ingroup does little to link

¹²All data were collected before the Boston Marathon bombing on April 15th, 2013.

¹³Throughout this paper, I convert the log odds coefficients of of logit regressions into more interpretable percentage point changes in odds ratios.

¹⁴Removing the observations with lowest and the highest WICIAT scores attenuates this result slightly and the coefficient on White Identity Centrality, for subjects exposed only to black images, is significant at the 10 percent level, not the 5 percent level.

ingroup identity to attitudes about due process. Exposure to violence by an outgroup, however, appears to cause a significant increase in the salience of white identity with regard to issues of safety and justice. Second, in contrast to the predictions of Sidanius and Pratto (2001), in this case it appears that perceptions of apolitical violence by an outgroup, as compared with political violence, may be especially critical in heightening the salience of white identity with regard to punitiveness.¹⁵

The results in Table 3.5.2 also speak to a larger debate in social science about how violent protests are categorized. Terms like “riot” and “rebellion” convey different interpretations about whether a movement or event was “mere criminality” or a political “uprising.” In the former case, the results in Table 3.5.2 suggest increasing White Identity Centrality is strongly associated with favoring greater punitiveness and, in the latter, no effect at all. These results also have important implications in the context of the political violence in the United States in the 1960s and the rising punitiveness of criminal justice policy in the decades that followed. These results suggest that violent protests by whites over issues like the Vietnam war may have been far less important than perceptions of rising apolitical violence by blacks at moving public opinion on criminal justice issues. Finally, these results offer insight into why it is often expedient for opponents of a movement to conflate violent protests with apolitical criminal activity.

3.5.2 DOES WHITE IDENTITY CENTRALITY INDIRECTLY INFLUENCE BEHAVIOR?

¹⁵That political violence by an outgroup did not increase the salience of white identity in this case may be an artifact of several distinctive aspects of this study. For example, political slogans like “fight for freedom” or “no to tyranny” may prime subjects to be more concerned with issues of due process with suspected terrorists. The slogans may also be perceived to support right-of-center values and, therefore, dampen anti-black affect by countering stereotypes of blacks as liberals. In the future I plan to run a sentiment analysis of the slogans to test whether they are perceived to have a liberal or conservative skew.

The unfair ultimatum game experiment suggests ways in which racial identification, as measured by WICIAT, might influence actions as well as opinions. In a conventional ultimatum game, two players decide how to split a fixed sum of money. The first player, or proposer, decides how to split the the money and the second player, or responder, chooses to accept or reject the offer. If the offer is accepted, both players receive their respective split of the money. If the offer is rejected, both players receive nothing. A rejection is typically understood as a costly way for the second player punish the first for making an unfair proposal. Across many ultimatum game experiments, the modal offer made by proposers is to split the money 50–50. Offers of less than 30 percent of the total sum are typically rejected by about half of the responders (Yamagishi et al., 2012; Camerer, 2003).

In the unfair version of the ultimatum game, subjects began by entering a first name or alias to use in the course of the game play.¹⁶ The subjects were then asked to wait a few moments while another “player” was enrolled into the game and the respective roles of proposer and responder were assigned. In actuality, the co-player was not a person but rather a software script and the subject was always assigned the role of second player or responder. In addition, the computer co-player always made the same low offer to split a \$1.00 pot with \$0.90 to the proposer and \$0.10 to the subject.¹⁷¹⁸

How is White Identity Centrality associated with the probability of accepting an unfair

¹⁶To maintain complete anonymity of subjects, this first name information is not linked to the subject’s responses.

¹⁷Though ten cents is not a lot of money relative to the median American income, it represented a possible 11 percent bonus for a single keystroke in a task that subjects knew would take about 20 minutes and for which they’d be paid at a rate of about \$0.04 a minute.

¹⁸In a manipulation check presented at the end of the survey, 86 percent of the respondents indicated that they found the offer to be unfair, 3 percent indicated they found the offer to be fair and 11 percent indicated they were uncertain.

Table 3.5.3: White Identity on Accepting an Unfair Offer in an Ultimatum Game, by Exposure to White & Political, White & Apolitical, Black & Political and Black & Apolitical Images

	White (All)	White & Political	White & Apolitical	Black (All)	Black & Political	Black & Apolitical
White Identity Centrality	0.77*	0.67	0.88	-0.48	0.13	-1.24*
	(0.37)	(0.51)	(0.54)	(0.34)	(0.48)	(0.53)
<i>N</i>	182	94	88	187	94	93
AIC	239.80	127.34	116.35	250.26	125.77	123.80
BIC	265.44	147.68	136.16	276.11	146.12	144.06
log <i>L</i>	-111.90	-55.67	-50.17	-117.13	-54.89	-53.90

Standard errors in parentheses

* indicates significance at $p < 0.05$

offer in an ultimatum game? Independent of the WICIAT score, subjects accepted the unfair offer on average 38 percent of the time. Table 3.5.3 presents the results of six logistic regressions in which the predictor is White Identity Centrality and the outcome is whether subjects accepted or rejected the unfair offer. The first three columns of Table 3.5.3 present the results for various conditions of exposure to white images. The last three columns present the same model but for those observations exposed to black images. Exposure to all white images (but not the white political or white apolitical conditions independently) causes White Identity Centrality to be positively and significantly associated with accepting an offer in the unfair ultimatum game. For the median subject exposed to white images, a one unit increase in White Identity Centrality associated with an 117 percent increase in the odds of accepting an offer. Conversely, exposing subjects to black and apolitical images (but not black images generally or black

political images) causes White Identity Centrality to be negatively and significantly associated with accepting an offer in the unfair ultimatum game. For the median subject exposed to black and apolitical images, a one unit increase in White Identity Centrality is associated with a 71 percent decrease in the odds of accepting an offer.

Figure 3.5.5 presents a plotted version of the results in column 1 of Table 3.5.3. Figure 3.5.5 is a scatterplot, for subjects exposed to white images, of the relationship between White Identity Centrality and accepting the unfair offer. The smoothed line indicates the predicted probability of accepting the offer as White Identity Centrality increases (with a 95 percent confidence interval in darker gray). As in Table 3.5.3, for subjects exposed to the white images, increasing White Identity Centrality is associated with a greater willingness to accept an unfair offer. By contrast, Figure 3.5.6 plots the results seen in column six of Table 3.5.3 and shows that, for subjects exposed to black and apolitical images, increasing White Identity Centrality is associated with a sharply decreasing probability of accepting an unfair offer. Put simply, exposure to white images causes white identity to be more associated with generosity and exposure to black and apolitical images causes white identity to be associated with punishment.

3.6 DOES EXPOSURE TO RACIALIZED AND POLITICIZED IMAGES DIRECTLY CHANGE THE “TASTE FOR PUNISHMENT?”

Beyond white identity, what direct effect do violent and politicized images of white and black men have on subjects’ propensity to punish when presented with an unfair offer in an ultimatum game? Additionally, how do those acceptance rates change when subjects receive a cue that their co-player is a co-ethnic or a member of a racial outgroup? These

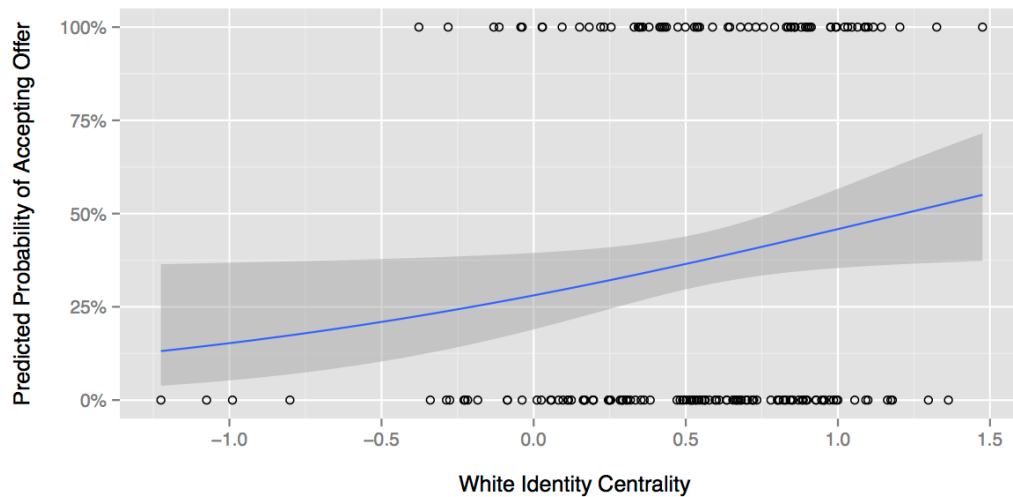


Figure 3.5.5: Among subjects who have been exposed to white images, irrespective of whether the images are political or apolitical, increasing White Identity Centrality is associated with an increasing probability of accepting an offer in the ultimatum game. The blue line is a smoothed logistic regression line estimating the predicted probability of accepting an offer as White Identity Centrality increases. The grey area around the blue line is the 95 percent confidence interval.

two questions inform the taste for punishment experiments.

As mentioned previously, in the unfair ultimatum game experiment, subjects respond to a proposal in which \$1.00 is split two ways such that \$0.90 would go to the proposer and \$0.10 would go to the subject. Under the rules of the game, rejecting such an offer results in neither player receiving any payment. Given that \$0.10 is better than nothing, a purely rational response would be for the subject to always accept even a low or unfair offer. The rejection of an unfair offer is typically interpreted as a way for the responder to punish an unfair offer from the co-player and, so, is a useful instrument to gauge whether the exposure to images with cues about race, violence and politics influences subjects' punitiveness.

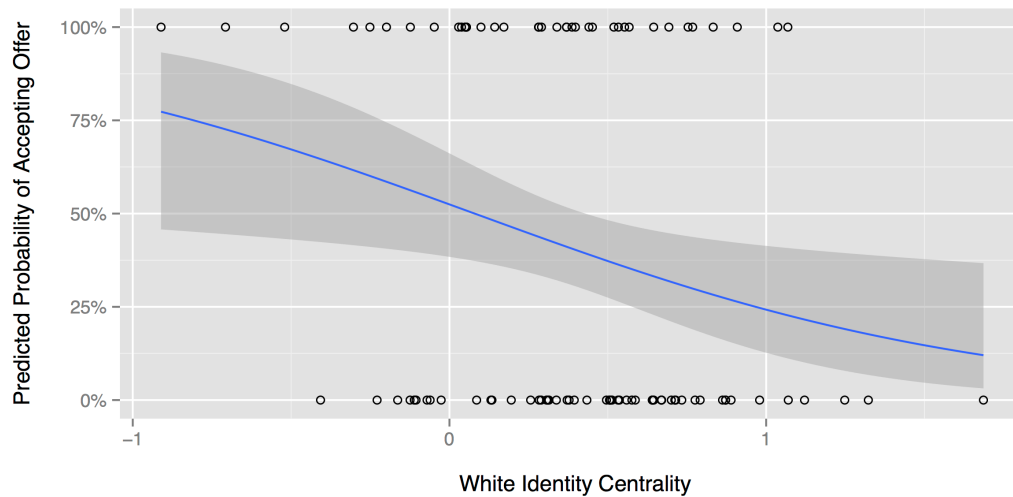


Figure 3.5.6: Among subjects who have been exposed to black and apolitical images (but not black and political images), increasing White Identity Centrality is associated with a sharply decreasing probability of accepting an offer in the ultimatum game.

3.6.1 THE TASTE FOR PUNISHMENT AGAINST ‘TODD’ AND ‘JAMAL’

In addition to receiving an unfair offer, subjects are randomly assigned a co-player with either a first name typical of white men, a first name typical of black men, or, in the control condition, “your partner.” For example, one subject might be told the co-player is named “Todd,” while another subject might be told the co-player is named “Jamal.”¹⁹ The name experiment evaluates whether receiving a signal about race changes the acceptance and punishment rates for “white,” or “black” co-players as compared to a racially unidentified co-player.

¹⁹The names are taken from Bertrand and Mullainathan (2004). To identify names that signal race, Bertrand and Mullainathan collect baby name data from Massachusetts birth certificates between 1974 and 1979. They define racially distinctive names as, “those that have the highest ratio of frequency in one racial group to frequency in the other racial group” (p. 995). They further validate the racial distinctiveness of the names with a small survey in Chicago.

Table 3.6.1: Summary Statistics of Effects of Images and ‘White’ Name as Co-Player on Accepting an Unfair Offer in an Ultimatum Game

	Variable	Condition 1	Condition 2	N	Mean	Min	Max	SD
1	Accept offer	All images	‘White’ name	118	0.44	0.00	1.00	0.50
2	Accept offer	White images	‘White’ name	61	0.48	0.00	1.00	0.50
3	Accept offer	Black images	‘White’ name	57	0.40	0.00	1.00	0.49
4	Accept offer	Political images	‘White’ name	59	0.47	0.00	1.00	0.50
5	Accept offer	Apolitical images	‘White’ name	59	0.41	0.00	1.00	0.50
6	Accept offer	White & political images	‘White’ name	30	0.47	0.00	1.00	0.51
7	Accept offer	White & apolitical images	‘White’ name	31	0.48	0.00	1.00	0.51
8	Accept offer	Black & political images	‘White’ name	29	0.48	0.00	1.00	0.51
9	Accept offer	Black & apolitical images	‘White’ name	28	0.32	0.00	1.00	0.48

Table 3.6.1 presents the summary statistics of the effects of exposing subjects to signals about race, violence and politics on their propensity to accept or reject an unfair offer from a co-player with a typically white name. As indicated in Row 1 of Table 3.6.1, across all subjects playing the unfair ultimatum game against a ‘white’ player, the mean acceptance rate was 44 percent. The remaining rows of Table 3.6.1, with the exception of the row for the black and apolitical condition, show little variation. Subjects exposed to the black and apolitical images (Row 9) accepted only 32 percent of the unfair offers.

Table 3.6.2 presents the results of six logistic regressions in which the name of the co-player is the predictor variable and accepting or rejecting an unfair offer in the ultimatum game is the outcome. For all observations in Table 3.6.2, half of the subjects were randomly assigned typically white names as the co-player and half of the subjects

Table 3.6.2: Logistic Regression of Effect of Images and ‘White’ Co-Player on Accepting an Offer in an Unfair Ultimatum Game (as Compared to ‘Your partner’)

	White (All)	White & Political	White & Apolitical	Black (All)	Black & Political	Black & Apolitical
White Identity Centrality	1.04*	1.21*	0.92	−0.01	0.92	−1.12*
	(0.39)	(0.59)	(0.53)	(0.37)	(0.52)	(0.56)
<i>N</i>	123	59	64	121	66	55
AIC	157.03	75.02	85.62	167.34	87.35	75.66
BIC	179.52	91.65	102.89	189.71	104.87	91.72
log <i>L</i>	−70.51	−29.51	−34.81	−75.67	−35.67	−29.83

Standard errors in parentheses

* indicates significance at $p < 0.05$

played against ‘your partner.’²⁰ Echoing earlier results, exposure to white images and a white name causes greater acceptance of an unfair offer. For subjects exposed to black images and a ‘white’ name (as compared to ‘your partner’), the coefficients on the political and apolitical conditions diverge. Though not statistically different from zero, subjects exposed to black political images exhibit more generosity for typically white names relative to ‘your partner’ and subjects shown black apolitical images are, once again, significantly more punitive. To visualize these results, see Figure 3.8.2 in the Appendix.

How do subjects respond when their co-player is ‘black’? Table 3.6.3 offers summary statistics for the effects of images on rates of accepting an unfair offer when a subject is presented with a ‘black’ co-player. Table 3.6.4 presents six logistic regression coefficients

²⁰Identifying an appropriate control for cues about race is difficult as subjects in a majority white country may consciously or unconsciously assume ‘your partner’ is white and, therefore, project race on to the co-player even when no explicit racial cue is provided.

Table 3.6.3: Summary Statistics of Effects of Images and ‘Black’ Name as Co-Player on Accepting an Unfair Offer in an Ultimatum Game

	Variable	Condition 1	Condition 2	N	Mean	Min	Max	SD
1	Accept offer	All images	‘Black’ name	125	0.37	0.00	1.00	0.48
2	Accept offer	White images	‘Black’ name	59	0.41	0.00	1.00	0.50
3	Accept offer	Black images	‘Black’ name	66	0.33	0.00	1.00	0.48
4	Accept offer	Political images	‘Black’ name	63	0.40	0.00	1.00	0.49
5	Accept offer	Apolitical images	‘Black’ name	62	0.34	0.00	1.00	0.48
6	Accept offer	White & political images	‘Black’ name	35	0.46	0.00	1.00	0.51
7	Accept offer	White & apolitical images	‘Black’ name	24	0.33	0.00	1.00	0.48
8	Accept offer	Black & political images	‘Black’ name	28	0.32	0.00	1.00	0.48
9	Accept offer	Black & apolitical images	‘Black’ name	38	0.34	0.00	1.00	0.48

for the effect of having a ‘black’ co-player on accepting an unfair offer (in contrast to ‘your partner’). As with the earlier results, white images, particularly white and political images, cause higher rates of acceptance. And, similarly, the signs on the black political and black apolitical condition diverge with the black apolitical condition, once again, causing a negative and statistically significant change in the acceptance rate.

Perhaps surprisingly, the differences in acceptance of an unfair offer between subjects randomly assigned a ‘white’ co-player and a ‘black’ co-player are not statistically significant. Table 3.6.5 presents the estimated effects of exposure to various combinations of images and being assigned a ‘white’ co-player as compared with a ‘black’ co-player. None of the coefficients in Table 3.6.5 are statistically different from zero.

There results of the unfair ultimatum game experiments are also important theoretically. First, the greater willingness of subjects to accept an unfair offer when

Table 3.6.4: Logistic Regression of Effect of Images and ‘Black’ Co-Player on Accepting an Offer in an Unfair Ultimatum Game (as Compared to ‘Your partner’)

	White (All)	White & Political	White & Apolitical	Black (All)	Black & Political	Black & Apolitical
‘Black’ Name	0.76 (0.40)	1.17* (0.57)	0.29 (0.58)	−0.31 (0.36)	0.25 (0.55)	−1.03* (0.52)
<i>N</i>	121	64	57	130	65	65
AIC	152.34	81.83	73.23	174.48	82.35	89.32
BIC	174.70	99.10	89.57	197.42	99.74	106.72
log <i>L</i>	−68.17	−32.92	−28.61	−79.24	−33.17	−36.66

Standard errors in parentheses

* indicates significance at $p < 0.05$

Table 3.6.5: Effect of Images and ‘White’ Co-Player on Accepting an Offer in an Unfair Ultimatum Game (as Compared to ‘Black’ Co-Player)

	White (All)	White & Political	White & Apolitical	Black (All)	Black & Political	Black & Apolitical
‘White’ Name	0.28 (0.37)	0.04 (0.50)	0.63 (0.56)	0.30 (0.38)	0.68 (0.55)	−0.09 (0.53)
<i>N</i>	120	65	55	123	57	66
AIC	168.14	93.72	77.50	164.90	79.33	87.99
BIC	190.44	111.11	93.55	187.40	95.68	105.51
log <i>L</i>	−76.07	−38.86	−30.75	−74.45	−31.67	−35.99

Standard errors in parentheses

* indicates significance at $p < 0.05$

primed with white images supports arguments suggesting at least some aspects of racial threat may operate as ingroup bias or affinity rather than solely as outgroup antipathy. Second, the greater likelihood that subjects would punish an unfair offer when primed with black and apolitical images suggests, nevertheless, that outgroup antipathy is still a significant factor in group threat. Third, the significant role of political slogans in increasing the propensity to accept for both ‘white’ and ‘black’ names suggests that political messaging does matter significantly in how people interpret what is fair (or at least acceptable). Fourth, though subjects were more likely to accept an unfair offer from a ‘white’ co-player than a ‘black’ co-player, the results were not statistically significantly different from zero. Given that subjects were significantly more punitive against a co-player identified only as ‘Your partner,’ the lack of differentiation between ‘white’ and ‘black’ co-players suggests that, at least in the context of this online lab experiment, the cue of a racialized name is less powerful than other sorts of outgroup bias.

3.7 DISCUSSION

Do violent protests trigger a “circle the wagon” mentality? The results of this experiment offer tentative support while raising additional questions. First, as predicted by Wasow (2012), exposure to violent and political media can increase the salience of white identity. However, contrary to prediction, subjects exposed to images of the black outgroup exhibited *lower* measures of ingroup identification than did subjects exposed to images of the white ingroup. Second, as predicted, heightened identity was associated with greater punitiveness. The association between heightened identity and punitiveness, however, was not uniform across treatment conditions and operated only for those primed by

apolitical images of the black outgroup. Third, though exposure to black images was associated with an increase in the percentage of respondents who identified themselves as conservative (as opposed to liberal), contrary to the prediction of the “circle the wagons” model, the effects of exposure to various images did not shift ideology at conventional levels of statistical significance (see Figure 3.8.1 in the Appendix).

Beyond the case of the violent protests in the 1960s, this experiment explores whether racial identity should be seen as something fixed and static or, potentially, more dynamic and responsive to stimuli. In the context of this paper, racial identification can be viewed as having two meanings. The first, already discussed at length, involves an individual’s sense of belonging to a group. The second, more academic meaning, engages the literature on causal inference and the challenge of identifying a causal effect of race. A widely held view in statistics is that race should be considered an “immutable characteristic” that cannot be randomly assigned and therefore cannot be isolated or identified as a “treatment.”

Building on the theoretical framework outlined by Sen and Wasow (2012), this paper demonstrates two problems with the traditional view of causal inference with race. First, some effects of race operate as a form of information that sends a signal to a person or institution. The signaling effect of race, as demonstrated in this study with images and names, is clearly amenable to experimental research designs. Second, in contrast to arguments that race should be conceptualized as a coherent, monolithic and fixed trait, this study demonstrates that an individual’s degree of racial identification changes in response to stimuli and that these changes have substantive effects on attitudes and behavior. In short, racial identification matters in politics and is possible in statistics.

3.8 APPENDIX

3.8.1 SLOGANS USED IN THE POLITICAL PODT TREATMENT:

1. Fight back
2. Rebellion Time
3. Revolution Time
4. Freedom or Death
5. Depose Tyranny
6. Wake Up!
7. Don't Tread on Me
8. Protect My People
9. Liberty or Death
10. Join the Resistance
11. No to Tyranny
12. Fight for Freedom
13. Ballot or Bullet
14. Revolution is Brewing
15. Declare Independence
16. My Voice Or My Gun
17. It's Us Against Them
18. Revolt
19. My People, My Nation
20. Take Our Country Back

3.8.2 SUMMARY STATISTICS OF EFFECTS OF IMAGES AND NAMES AS CO-PLAYERS ON ACCEPTING AN UNFAIR OFFER

Table 3.8.1: Summary Statistics of Effects of Images and ‘White’ names, ‘Black’ Names and ‘Your partner’ as Co-Players on Accepting an Unfair Offer in an Ultimatum Game

	Variable	Condition 1	Condition 2	N	Mean	Min	Max	SD
1	Accept offer	All images	All names	369	0.38	0.00	1.00	0.49
2	Accept offer	All images	‘white’ name	118	0.44	0.00	1.00	0.50
3	Accept offer	All images	‘black’ name	125	0.37	0.00	1.00	0.48
4	Accept offer	All images	‘Your partner’	126	0.33	0.00	1.00	0.47
5	Accept offer	White images	‘white’ name	61	0.48	0.00	1.00	0.50
6	Accept offer	White images	‘black’ name	59	0.41	0.00	1.00	0.50
7	Accept offer	White images	‘Your partner’	62	0.24	0.00	1.00	0.43
8	Accept offer	Black images	‘white’ name	57	0.40	0.00	1.00	0.49
9	Accept offer	Black images	‘black’ name	66	0.33	0.00	1.00	0.48
10	Accept offer	Black images	‘Your partner’	64	0.41	0.00	1.00	0.50
11	Accept offer	Political images	‘white’ name	59	0.47	0.00	1.00	0.50
12	Accept offer	Political images	‘black’ name	63	0.40	0.00	1.00	0.49
13	Accept offer	Political images	‘Your partner’	66	0.24	0.00	1.00	0.43
14	Accept offer	Apolitical images	‘white’ name	59	0.41	0.00	1.00	0.50
15	Accept offer	Apolitical images	‘black’ name	62	0.34	0.00	1.00	0.48
16	Accept offer	Apolitical images	‘Your partner’	60	0.42	0.00	1.00	0.50

3.8.3 DIFFERENCE IN MEANS PLOT FOR EFFECT OF IMAGES ON IDEOLOGY

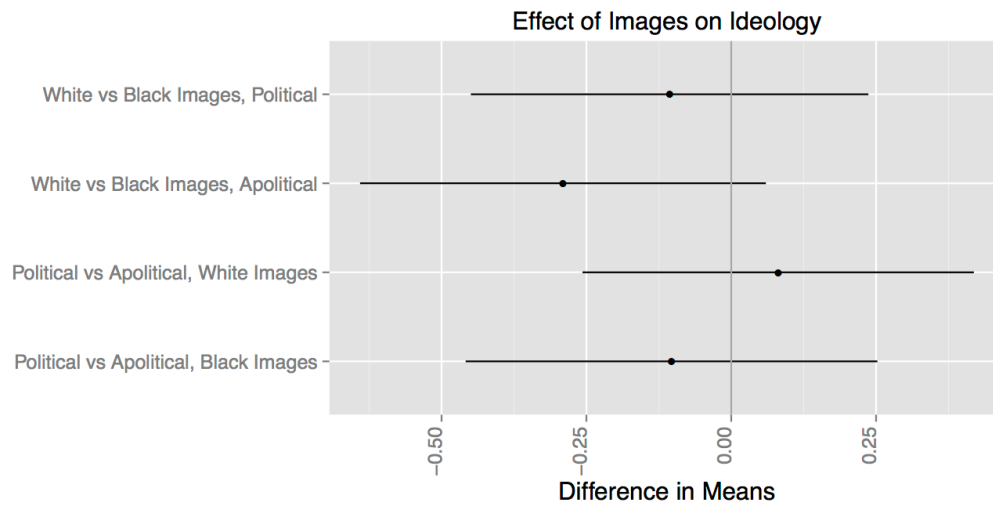


Figure 3.8.1: Difference in Mean Plots for Effect of Images on Ideology

3.8.4 DIFFERENCE IN MEANS PLOT FOR EFFECT OF IMAGES ON ACCEPTING FOR 'WHITE' NAME VS 'YOUR PARTNER'

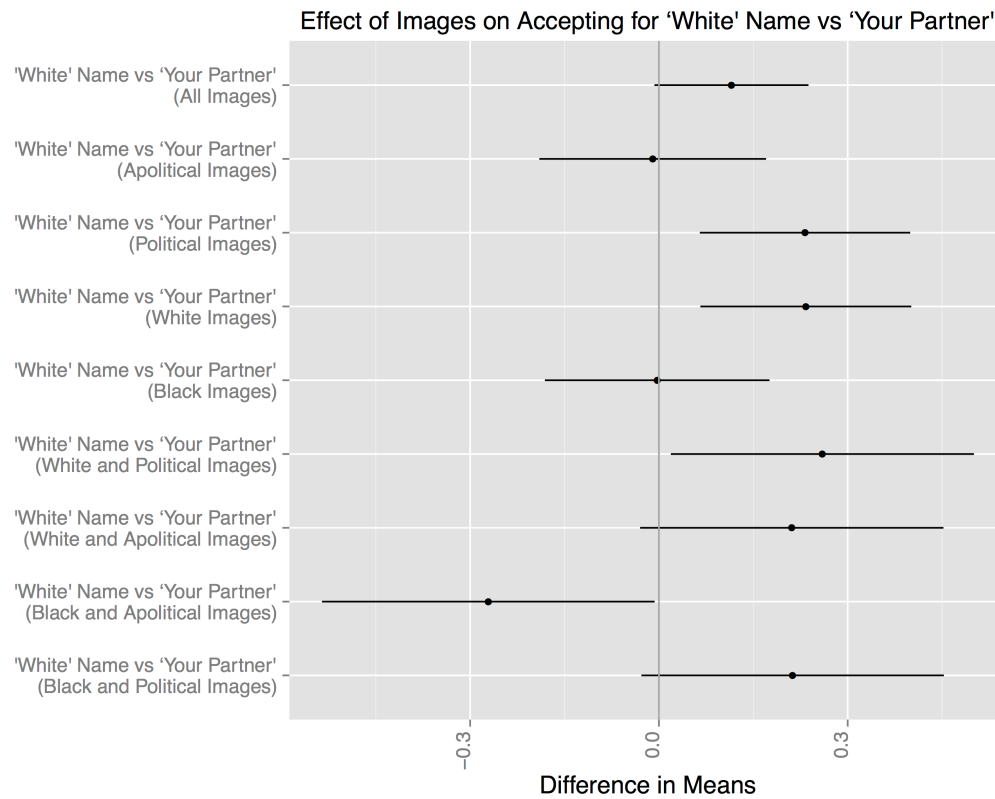


Figure 3.8.2: Difference in Means Plot for Effect of Images on Accepting for 'White' Name vs 'Your Partner'

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Colophon

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